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PATRON: HIS MAJESTY KING GEORGE VI.

[*Continued on page (III) of Cover.*

ELECTRIC HEATING FOR MERCHANT SHIPS

By H. CAMDEN MACEWAN, Associate Member.*

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SUMMARY

A brief review of the differences between heating requirements at sea, and shore heating installations, opens the subject, and the paper proceeds to examine the heat losses on board ship and the heat-loss coefficients available for the calculation of quantities. The existence of internal heat output and reserves is then dealt with; ventilation losses are discussed; and the special conditions existing in the public rooms of passenger ships are reviewed.

After considering the methods of heating and the growth of the use of electricity for that purpose, the rules and regulations controlling the design and installation of electric heaters on board ship are reviewed. The question of radiant versus convection heating is discussed; also the question of fire risk and the difficulties found in the application of radiant heating.

The use of thermostats and the cost of thermostatic control is considered, and the paper closes upon the question of the testing of heating installations on board new ships.

The conclusions drawn from this review are that calculation of quantities is possible, and that there is room for development in the application of electric heating to ships.

INTRODUCTION

The heating installation of a merchant ship has nothing of the seasonal nature associated with shore heating equipment, for it may, and usually will, be brought into service frequently throughout the year and forms on most trades an essential and constantly used part of the ship's equipment. For an ocean-going merchant ship may sail into cold weather at any time of the year; in the course of less than a week she may sail from tropical heat into almost arctic conditions; and in a voyage of less than a month's duration may sail from winter in one hemisphere through spring, summer, and autumn, or vice versa, and reach winter conditions in the other hemisphere.

Further, temperature conditions are, in general, cooler at sea than they are on land; the vessel is continually subjected to the natural cooling arising from her speed of 20 miles an hour or so; and the air is more humid and therefore "rawer" than is commonly found ashore. In consequence heating is more quickly in demand when the outside temperature drops, and is more frequently in demand under similar conditions of outside temperature.

The heating installation for a passenger liner of to-day may represent an electrical load of 1 000 kW or more, and the factors in the design of such heating installations differ considerably from those pertaining to the heating of a building. It is true that the amount of heat necessary to heat a space on board ship may be summarized as the total of the components necessary:—

(1) To heat the structure, furnishings, and air, making up the space.

(2) To make good the heat losses by radiation and dissipation from and through the various parts.

(3) To make good the heat carried away by ventilating air and the opening and closing of outside doors.

Such a summary, however, although doubtless correct in theory, leaves in obscurity factors of sea-going conditions which to a large extent control quantities and design.

The difficulty of arriving at a simple method of calculation on a basis similar to that used for buildings is illustrated in the fact that although the various authorities and regulations require the shipowner and shipbuilder to provide "proper" and "adequate" heating of such and such spaces, none of them ventures to lay down what they consider to be "proper" or "adequate" even in terms of watts per cubic foot when they have electric heating in mind. Indeed it is only within the last few months that as much as a temperature-rise has been mentioned, the British Board of Trade having led the way by saying in their latest instructions to their surveyors in relation to masters' and crews' spaces that "a heating system will be considered satisfactory if it is . . . capable of maintaining a temperature of 60° F. when the temperature of the outside air is 30° F." While this may be a useful indication for calculations it is obviously insufficiently defined to form the basis of tests to show the adequacy of the heating, and it brings us once more up against that hitherto debatable point, but nowadays quite established fact, that the air temperature is not a true measurement of proper and adequate heating.

Up to the present, in heating a ship electrically, the amount of heat in kilowatts installed in the various spaces has been somewhat arbitrary, but shipowners and the various regulating authorities are beginning to show a tendency to look more closely into the question and, further, to take the view that it is better to provide too much heat rather than too little. If cost is accepted as a secondary consideration and no fire risk arises, that point of view is perfectly sound; at the same time it should be possible to establish some suitable basis for estimating the amount necessary to secure adequate heating without overdoing it. It is important that some such basis should be established, for a heating installation on a ship not only involves appreciable capital outlay, running costs, and maintenance costs, but also if it is overdone it may call for additional generator capacity for heating alone, which would of course make the cost of electric heating prohibitive.

As a general rule the amount of heat provided with electric heating has been sufficient. At the same time

* Messrs. Harland and Wolff, Ltd.

it has not always been sufficiently generous, and grumbles against electric heating have been heard after a particularly cold spell has been met. The object of this paper is, therefore, to gather together the data available for the calculation of quantities, to review the problem generally as it exists to-day, and to promote discussion which will put the design of the electrical heating of ships upon a sounder basis.

AMOUNT OF HEAT REQUIRED. HEAT LOSSES. BARE STEEL

The most serious source of heat loss on a ship is uncovered steelwork. Glass windows, which are usually the most serious factor in buildings, are very small on board ship, except in the public rooms around the promenade decks, and may without significant error be treated as part of the steel in which they are fixed. It will be realized at once that uncovered steelwork is prevalent in quite large areas on all classes of ships, for, apart from the public rooms, the best staterooms, and the captain's and chief engineer's quarters, it is not usual to panel the ship's sides or the deck overhead. Even in 1st-class accommodation the deck overhead is quite often left bare, that is to say it is merely painted or cork-dusted, although the ship's sides may be panelled.

The losses through these metal walls and ceilings may be considerable, for in addition to heat being conducted through the thickness of the plate to the outside atmosphere there may also be appreciable transference of heat through the metal structure from high- to low-temperature regions. In the comparatively still air of average land conditions that would be serious enough, but at sea such surfaces may literally be air- and water-cooled; in fact, the conditions on the outside surface may be so bad that a layer of ice upon it—which may not infrequently occur—will be beneficial from the point of view of heat retention. Ships that go no farther north than New York or the Baltic experience conditions of 20 degrees of frost (12° F.), and the heat loss through a bare metal bulkhead or ship side with no more heat insulation than a layer of paint and cork-dust, which is being pounded and washed with solid sea around freezing point and cooled with a gale at "twenty below," is, it will be readily realized, very large indeed.

Some rooms have three sides of the six of such bare metal, and although the deck overhead will probably be covered with wooden deck planking or by the floor of another space, the corner room of a deckhouse may have on two sides nothing between its atmosphere and the outside weather but half an inch of steel (see Fig. 1).

It is obvious that on a ship that sails into severe temperature conditions the cost of panelling such bare metal would be saved over and over again, but sometimes the various authorities object to panelling on account of its proclivities for harbouring and encouraging the spread of rats and other vermin. The losses must, therefore, be met and remedied on some ships with heating capacity.

Later in the paper a calculation is made of the heat losses in a room with a large proportion of bare steel, but meantime, in case it may be thought that the severities of the conditions and the losses that arise from them have been exaggerated, the following report from the captain of a small cargo ship after a voyage to the Baltic will show what is actually experienced, and it may be said that the conditions he records are not in any way abnormal for winter in that trade, also that the ship was more than well heated electrically, according to accepted standards:—

"The temperature at Hamburg," he wrote, "was 13 to 16 deg. F. below freezing point. In the crew's quarters sheets of ice formed on the ship's side and the decks were wet continually"—(he refers to internal ice and condensation moisture)—"and the heaters were so inadequate that the men could not undress as their quarters were so intensely cold. In the bo'sun's cabin a sheet of ice formed at the foot of his bunk. The officers' cabins were not quite so bad, though personally I had to keep my bridge overcoat on when in my cabin in order to keep sufficiently warm."

The most striking feature of that report is that the ship referred to was considered at the time of building to be considerably overheated. The bo'sun's cabin in which

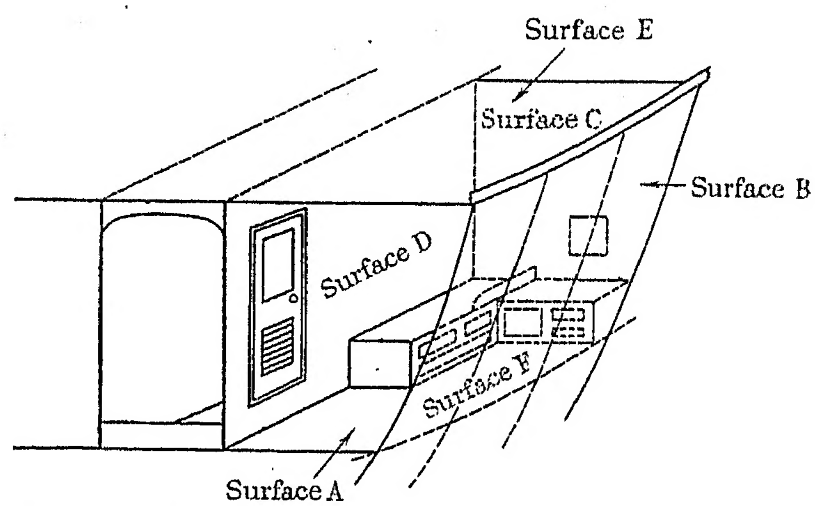

Fig. 1.—Outline view of a petty officer's cabin in the fo'c'sle of a merchant ship.

Surface A.—Bare steel bulkhead to well deck and atmosphere.
,, B.—Bare steel ship's side exposed to weather.
,, C.—Bare steel bulkhead exposed to next cabin.
,, D.—Bare steel bulkhead exposed to open alleyway.
,, E.—Fo'c'sle deck, wood planking on steel.
,, F.—Composition decking on steel to hold below.

the ice formed internally at the foot of the bunk was heated to the extent of over $4\frac{1}{2}$ watts per cubic foot. Its volume was only 450 cu. ft., disregarding the furnishings, the air volume being under 400 cu. ft., and it had a 2-kW heater fitted. The sides of the cabin were all bare steel. In the captain's and officers' rooms, which we note were more comfortable, the heating was less, averaging 3·0–3·3 watts per cu. ft., and the crew's quarters elsewhere had 3·75–4 watts per cu. ft. The ship had left the builders' hands during winter in this country, and the heating had been fully tested both before leaving and during trials. It had been found quite impossible to keep the heaters full on for any length of time in port, for the rooms became unbearably hot; and in one room, when the heater was left on inadvertently all night, all paintwork and polish were destroyed. (See Table 5 for a calculation of the heating in this bo'sun's room.)

The heat loss through steel of normal thickness used as part of a ship's structure and painted on both sides, would in still air and normal winter conditions be round about 1·0 B.Th.U. per square foot per hour per degree F. difference of temperature, but observation indicates that under average winter conditions at sea that figure should

be increased to 1·8. Under very severe conditions such as are experienced in the Baltic and the North Atlantic in the winter 1·8 would be too small where the steel was fully exposed, and it would be unsafe to allow less than 2·5. This range of value may appear to be too great, but experience shows that such wide variations are actually experienced.

OTHER HEAT-LOSS COEFFICIENTS

The large range of coefficients which have been satisfactorily established for buildings, upon the results of authoritative experimental investigation, are in general applicable to ships' calculations, but they require a certain amount of adaptation to the different conditions of shipboard heating. For instance, in buildings it is usual to add 10 % to 15 % for exposure to directions other than south (in the northern hemisphere) and to make a similar allowance for exposure to the sky, but the consideration of northerly aspect cannot of course be brought into calculations for a ship. As a compromise, one side of the ship might be regarded as north and the other as south in calculating for large areas taking in the whole beam of the ship, but in dealing with a small area such as a corner room of a fo'c'sle, an officer's house on the boat deck, or an isolation hospital on the poop, the northerly factor would have to be used always. Again, with buildings another 15 % is sometimes added in dealing with structures in situations of extreme exposure such as the top of a cliff or outstanding hill, but as all ships are exposed to the four winds of heaven they must be regarded as being fully in that category.

The wide variation of temperature and weather conditions which has to be provided for in marine service tends to simplify calculations since they must on that account always be generous and could never be reduced to the degree of accuracy possible with buildings. The coefficients in Table 1 will be found to cover satisfactorily the usual range of calculations for ships, and it is of special interest to note the effect of lining a bare steel bulkhead or ship's side, as the saving in costs effected by such insulation will be seen to be very appreciable, bearing in mind that this saving will be not only in the installation costs but in the running costs also. The problem of securing a suitable lining for spaces where there is danger

Table 1

Surface	Heat-loss coefficients in B.Th.U./sq.ft./hr./deg.F.diff. under conditions as follows:—		
	Mild conditions	Full exposure to average winter wind and weather (say not more than 5 deg. to 10 deg. F. of frost)	Exposed to severe cold and weather as North Atlantic and Baltic
Steel			
Painted only, or covered with thin cork-dust and paint	1·0 to 1·8	2·0	2·5
Lined bulkhead, i.e. steel, with blocked air-space of 2–4 in. and ⅜–½ in. wood or composition board panelling ..	0·5	0·55	0·58
Public room, panelled bulkheads	0·4	0·44	0·46
Deck overhead with 3 in. wood deck planking to weather	0·45	0·5	0·52
Wood Partitions			
⅜ in. plywood bulkhead	0·43	—	—
Single-panel bulkhead	0·52	0·57	0·6
Double hollow teak or composition board bulkhead ..	0·35	0·39	0·4
Wood Doors			
Teak outside door	0·38	0·41	0·43
Whitewood inside door	0·45	—	—
Vestibule double doors	0·25	0·28	0·29
Plaster Work in public rooms, lining steel	0·4	0·44	0·46
Glass			
Portholes with brass frames	1·1	1·2	1·27
Public room, large windows to promenade deck ..	1·03	1·15	1·18
Public room, dummy windows without wood lining ..	1·0	1·1	1·15
Public room, dummy windows with bulkhead lined ..	0·6	0·66	0·69
Deck (Floor) Steel below			
2½ in. wood planking	0·35	0·39	0·4
1 in. composition decking	0·45	—	—
Cork slabs	0·27	—	—

of vermin is a difficult one which will have to be solved, and it is hoped that there will soon be produced some satisfactory form of insulation which can be effectively and securely painted or sprayed or plastered on to a ship's side to a thickness sufficient to reduce the transmission factor down to something in the region of 0·3–0·5 B.Th.U. per square foot per hour per deg. F. difference in temperature.

TEMPERATURE-DIFFERENCES AND HEAT RESERVES

The question of temperature-differences on the two sides of a surface must be carefully considered, for outside temperature is not an all-round basis for temperature-difference on board ship. There may be, and in fact usually are, a considerable number of different temperatures in different parts of the ship; internal bulkheads, for instance, may be quite hot from a galley or machinery space on the other side, a floor may have underneath it a cold store, or another space at an equal temperature or a machinery space which may or may not be hot, for motor engine-rooms sometimes need heating in cold weather. Again, a hold may be at almost any temperature according to circumstances. If it is a refrigerated hold the heat loss can easily be estimated, but it may be uninsulated and filled up with a bulky heat-holding cargo which was loaded in the tropics at a temperature of 100° F. and may then form a considerable reserve of heat, giving, of course, a negative heat-loss effect over an appreciable length of time.

Alleyways and entrances are items to be watched. In passenger accommodation it is fairly safe to assume that they will be at a temperature equal to the general temperature of the space, but in the case of officers' and crew's accommodation outside doors may be frequently opened, and due allowance must be made for that contingency. Some such alleyways cannot be taken as likely to be more than 10 to 15 deg. F. above the outside temperature.

In making calculations, therefore, it is necessary as a first step to scan on a plan of the ship the ambient conditions existing when heating is required.

The factor of heat reserves is often of considerable importance. Such reserves exist internally in almost all ships, but particularly in those that sail through the tropics. Under tropical sun the steelwork exposed to the direct rays will reach a temperature of about 150° F. or more, and inside the ship the side plating on which the sun is beating will be so hot that one cannot keep one's hand against it. The ventilating air pumped into the ship by fans and cowls will be around 90° to 95° F., and the hull below the waterline is immersed in water at a temperature of 80°–85° F. It will be readily understood that when this continues for some days the whole of the structure and furnishings will absorb and hold a large amount of heat which, it must be remembered, will be given up automatically when the vessel travels into cooler conditions; in fact this will provide for quite an appreciable time under the cooler conditions a remarkably complete system of mild panel-heating, especially in panelled rooms and in the vicinity of other reserves of heat such as the warm bulkheads already mentioned, and the holds full of warm material, where the heat will be well conserved and given out slowly.

This emission from reserve is very pronounced and makes it unnecessary on many trade routes, where a ship travels from merely cool conditions such as we have in Great Britain, into the tropics and out again, to provide as much heating as would be necessary on a North Atlantic liner or a Baltic coaster. In fact, ships sailing on a route more or less north to south and no farther into colder conditions than England or its equivalent in the south, seldom need generous treatment in designing the heating installation since they seldom get really cooled down. This is illustrated in the case of a vessel which sails due north and south, crosses the equator once a month, and goes far enough both north and south to find normal English weather in both hemispheres. Her owners cited her as an example of a well-heated vessel which had given complete satisfaction from the heating point of view throughout 10 years' service, yet the heating installation, which was electric, amounted to an average of only 0·62 watt per cu. ft. with a maximum figure of 0·85 and a low figure of as little as 0·55 watt per cu. ft. in some parts. The importance of route, therefore, apart from the conditions of extreme cold on the route, must be carefully taken into consideration as a prime factor. This vessel could not have passed the present Board of Trade test at the time of building in this country, but in service the crew were fully satisfied. There is, of course, the possibility with every passenger ship nowadays that she may be put on cruise at times, and under cruising conditions some of these heat reserves may be absent. As a general rule, however, pleasure cruises seek warmth, and conditions of severe cold may be regarded as abnormal on such services.

There does not seem to be any possibility, unfortunately, of laying down any principle as a basis for judgment or of settling any figure as a basis for ascertaining what allowance should be made. There are too many variables. All that can be done towards settling the matter is to weigh up the situation on a basis of route and the temperature conditions to be met on the route, the amount of time for which the vessel will be at the various temperatures, the nature of the cargo, and the temperature conditions under which it will be stowed, her general construction both as regards accommodation, furnishings and hold conditions, and arrive by careful judgment at its sum total effect.

The factor of heat emission from hot places on board, such as galley, pantries, boiler-room fiddleys, etc., is dealt with by ascertaining the temperature on the hot side of the bulkhead and calculating the heat gain accordingly.

Coming now to the question of the inside temperature to be maintained on board, this is always a matter of some controversy, as a result of the fact that air temperature is not a true measure of comfort conditions. As matters stand at the moment, however, it is the only recognized measurement. So far as the crew's accommodation is concerned, the B.O.T. standard of 60° F. must be used and it is quite sufficiently warm, for it must be remembered that some members of the crew will leave their rooms and go straight into the worst possible conditions of wind and weather, perhaps to stand in a completely exposed position for the whole of their

watch (4 hours). It is therefore no kindness to overheat the crew's accommodation. The advantages of radiant heat for such parts of the vessel will be considered later. In the passengers' accommodation, while $62\frac{1}{2}$° F. might be considered a fairly comfortable temperature to anyone usually resident in this country, it would be found distinctly cool to anyone accustomed to living in the tropics. Further, people from other parts of the world, notably Americans and Canadians, demand an inside temperature much higher, and such people are frequent passengers on board ship. In 1st-class accommodation on passenger vessels, therefore, 65° F. must be regarded as a minimum, and it is wise to aim towards at least 70° F.

VENTILATION LOSSES

The amount of ventilation varies considerably in various parts of the ship. The average full ventilation in 1st-class accommodation provides a stateroom with 10 to 15 changes of air per hour, but it is safe to assume that the amount will be cut down in cold weather both by the stewards and by the passengers themselves. Generally

Table 2

Space	Watts required for air-heating	
	For 30 deg. F. rise	For 50 deg. F. rise
Small stateroom 8 ft. square × 8 ft. high	300	500
Large stateroom 10 ft. square × 8 ft. high	400	700
Cabine de luxe 16 ft. × 12 ft. × 8 ft.	800	1 300
830 cu. ft. per hour	140	230
1 200 cu. ft. per hour	200	330

the ventilation to staterooms is supply ventilation and it blows out into the alleyways and travels thence to various parts, principally the lavatories and baths, from which it is drawn by exhaust fans. This warmed air from the staterooms may be relied upon to make the lavatories and baths fairly comfortable, and in this way some of the ventilation losses are picked up on their way out, as it were. Smokerooms, pantries, galleys, baths, and lavatories, all have exhaust ventilation and take a heavy toll of heat.

It is not easy to estimate ventilation losses, since they depend so much upon the personal element and upon where the warm air goes to before it is blown into the atmosphere. The output of the ventilating fans at, say, a minimum speed might be determined and taken as a basis, but it is uncertain how the openings in the trunks will be adjusted. There are also a large number of natural ventilation supply and exhaust cowls on board ship, particularly in the crew's quarters, which have a very real effect and from which the probable ventilation cannot be determined. The opening of outside doors, which do not, except in luxury public rooms, have any kind of vestibule to control the wintry blasts from outside, has also to be borne in mind.

For both passengers' and crew's accommodation a cold-weather ventilation of 3 changes per hour will generally be a satisfactory estimating figure, but for a smokeroom a higher figure, say 6 changes per hour, will be necessary. Of course the number of changes must depend to some extent, especially with small rooms, upon the cubic content of the room and the number of persons in occupation. For crew's rooms and emigrant accommodation 830 cu. ft. per hour per person is what the B.O.T. surveyor usually estimates as a minimum; there is no official figure for crew's spaces, but it is doubtful whether any crew's room gets as much as that in cold weather. For passenger accommodation the figure should be about 1 200 cu. ft. per hour per person.

As an average estimating figure it may be taken that 53 cu. ft. of air requires 1 B.Th.U. to raise it 1 deg. F. The figure varies with the state of humidity and with the temperature of the cold air, the figure given being a reasonable average.

As an indication of what these figures entail, Table 2 shows the amounts of electric heating (to the nearest 100 watts) required to heat air alone.

THE HEATING OF PUBLIC ROOMS

The heating of each public room on a passenger vessel requires individual consideration. There is infinite variety in the design and layout of these rooms to suit the taste and comfort of the people of all nationalities they are called upon to serve and please, and to suit the varied conditions of the ship's itinerary, whatever that may be. Hot-weather ships have rooms that are particularly airy and cool, notwithstanding the fact that they may at times need heating, but on a ship like a North Atlantic liner the rooms will be designed for long periods of winter conditions and will accordingly be a comparatively easier heating problem.

As a general rule, the rooms are low in height and the difficulties of effective distribution of heat are not, therefore, serious. Some rooms may be 15 to 20 ft. high, stretching through two decks, and some have large so-called domes of about that height in their centre, but the height of the majority is for the most part only about 10 ft. or so.

The factor of the lighting load in such low rooms is important, especially where cornice lighting is fitted, for a good proportion of the lighting is in service throughout the day—say 16 to 18 hours—the amount of natural lighting usually being insufficient. The heating from this source is appreciable, as will be seen from the following data of a room on a recent passenger ship:—

Floor dimensions	67 ft. × 80 ft.
Height	10 ft. 6 in.
Volume	57 500 cu. ft.
Lighting load (cornice lighting)	24·5 kW

The lighting in that room, it will be seen, represents heat to the extent of 0·426 watt per cu. ft., which is more than 75 % of the amount of heat installed for heating in a similar room on board the satisfactorily heated ship mentioned above.

Most of the public rooms are in the upper structure of the ship, and the amount of exposure to the weather varies greatly. Some are protected by weather screens round

the fore part of the promenade deck, while others may have all four walls and the deck above fully exposed. An interesting heating problem arises in the forward enclosed space mentioned, as this is sometimes furnished as a restaurant, observation lounge, or dancing space, in which heating is required. This is obviously a problem which can only be dealt with satisfactorily with radiant heat.

In regard to the number of people likely to be in a room at one time when heating is required, rooms such as the lounge or smokeroom may be empty or practically so for appreciable periods, and sufficient heat must therefore be provided to keep the empty room comfortable. In 1st-class and cabin-class rooms also it has to be remembered that at night some of the occupants will be ladies in light evening clothes, and a generous degree of warmth must therefore be provided. Dining saloons will frequently be filled to capacity and the heat from the occupants may be considerable. At breakfast time the passengers filter in and out and there are never very many in the room at one time, but as lunch and dinner are served at definite times the room may be full and heat regulation then becomes difficult. The following summarizes the situation in regard to dining saloons:—

(*a*) Sufficient heat must be provided for times when there are only a few passengers present.

(*b*) The room must be brought up to adequate temperature before the passengers arrive for the meal. This must be done by the heaters alone, but ventilating air may be reduced to a minimum while the room is heating up.

(*c*) With the passengers present there is the additional heat of 1 kW for each 9 people present, including waiters (whose output incidentally is probably about 500–600 B.Th.U. per hour instead of the 350 from a sedentary person). In addition there are probably a number of hotplates in use round the room, and the heat from the large amount of food is also not without effect.

(*d*) As the waiters have their attention concentrated upon the service, there is considerable chance of the room becoming overheated. This is sometimes dealt with by increasing the ventilating air, but that can of course only be done up to the point where incoming cold air creates draught.

It will be seen from this that there is a strong case for the use of thermostats.

The heating data of a dining saloon on a recent ship are as follows:—

Heating equipment ..	48 kW
Lighting load	24 kW
Heat from occupants ..	37 kW

METHODS OF HEATING

There are two methods of heating a ship:—

(1) By means of warmed air blown into the ship by fans and either merely heated or "conditioned."

(2) By means of heaters of various kinds which distribute heat either by radiation or convection or by a combination of both effects.

Heating by means of warmed air is undoubtedly very pleasant, particularly if the air is conditioned to the right degree to give the optimum degree of comfort at the required temperature, but it is an expensive method, for it involves costly heat-insulated air trunking to distribute the warmth, and costly plant if the air is conditioned. If the air is not conditioned it is inefficient as a result of large quantities of warmed air being blown into the atmosphere. It may be said that there is no need for it to be inefficient, and that the fans may be reduced in speed to the minimum output necessary for proper ventilation and no more heat used than is necessary for that amount. In practice, however, that ideal condition is not obtained. With air-conditioning plant some economy in heat is secured by re-circulation of the air after purification and added heating.

There can be no doubt that it is only a matter of time until air-conditioning is fitted on all high-class passenger ships, and probably it will soon be the accepted method of heating and ventilating the public rooms of passenger vessels. There is no reason why electric heating should not be used for such conditioning plants, but waste heat may be found to be a very strong competitor in designing such equipment, for most of the air-conditioning plant will be situated in the centre of the ship and will consequently be close to the hot exhaust gases in the funnel.

In considering waste heat, however, it has to be remembered that it ceases to be available in sufficient quantity when the main propulsion engines are shut down or slowed down to any appreciable extent, which may happen for days at a time in severe weather or fog. There is, therefore, an almost complete shutdown of the heat source when approaching and lying in port. Waste heat is not, therefore, a reliable medium and if it is used some stand-by medium is necessary.

Heaters may be of the steam, hot-water, or electric types. Coal and coke stoves are sometimes used on small ships, but it is only comparatively recently that electricity has come to be accepted as a suitable medium for the general heating of ships, apart from what may be termed luxury equipment. Its use has developed very rapidly, chiefly owing to the passing of its chief competitor. The use of electricity has always been attractive, of course, for actually it is cheap to install on board, cheap to run, and its upkeep costs are very much smaller than those of steam-heating installations, but until motor ships became a definite factor in ship design all ocean-going merchant vessels of any considerable size were steam-driven with steam at low pressure and with little or no superheat. It was therefore a simple matter to tap off heating steam from the propulsion supply, and the cost of it, apart from upkeep, was practically nothing more than the cost of the fuel to produce it. A motor ship, however, may not have steam, and on the high-pressure, high-superheat turbine-driven vessel with high-efficiency boilers, steam suitable for purposes outside the propulsion turbines may not be readily available, and even on ships with waste-heat boilers the amount of steam is not always sufficient to deal with the heating installation after other needs have been supplied. The heating medium of former days has, therefore, largely ceased to exist, and concurrently the expansion of the use of electricity throughout the ship has provided such extended generator capacity that it is usually sufficient to deal with the heating load, there

being no need to install extra generator capacity for that purpose alone.

After regarding steam heating for so long as almost the natural medium for heating a ship, the shipowner, and quite often the naval architect too, approaches the problem of heating a ship electrically with the aim to provide electrically as much heat as would have been given out by the steam heaters on a suitable steam-heating installation, and the question is often asked "How does this electric heater compare with a steam radiator of such and such a size?"

While it is impossible to make such a comparison, the question is a useful one in that it brings to our notice very prominently the fact that the action of a steam radiator is from the point of view of ship's heating very nearly ideal and very hard to equal with any other medium. In fact, as the use of electricity for heating grows it becomes more and more clear that developments

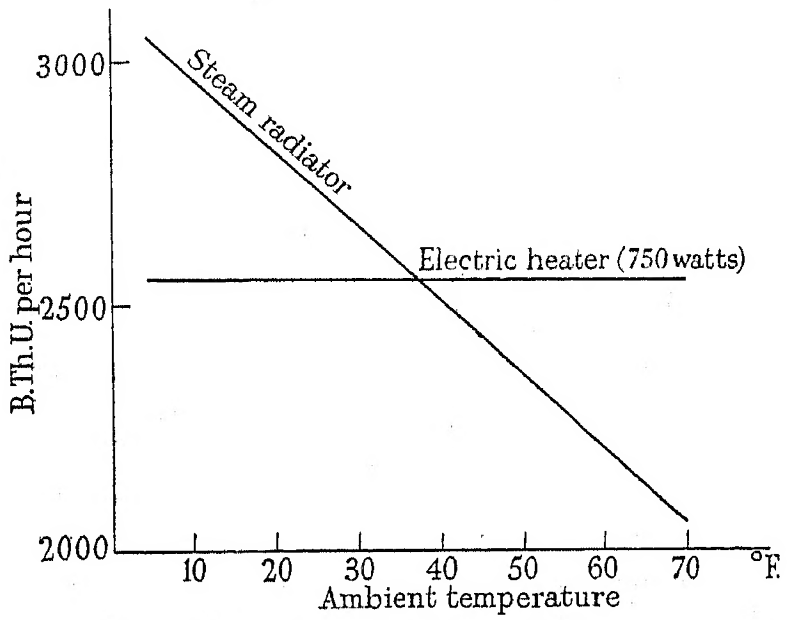

Fig. 2.—Curves of heat emission.

which will give the electric heater something of the same inherent self-adjustment to the ambient conditions are very necessary.

In the first place a steam radiator adjusts its heat output automatically to the conditions it has to deal with; that is to say, the colder its surroundings the more heat it gives out, provided of course that it can get all the steam it requires, and subject, of course, to ultimate limitations of size. As the room warms up it gives out less heat and if the temperature drops it gives out more; in fact there is in a steam radiator something of an inherent thermostatic effect which to a small but valuable degree regulates its output.

On the other hand an electric heater gives out a certain fixed amount of heat which no ordinary conditions of surrounding room temperature will alter (Fig. 2 shows the relative action of steam and electric heater with respect to ambient temperature).

It has of course the compensating advantage, which is sometimes very important, that its heat is always available in full measure since cold cannot freeze it nor throttle it, as may happen with a superabundance of condensate in a steam heating system. It must be realized, however, that this fundamental fact of the fixed amount of heat is of considerable importance. It makes it necessary to be generous in settling the size of the heaters to ensure that it is adequate to deal with the most severe conditions they will be called upon to remedy, and as a result of that generosity of size the capacity will be appreciably greater than is normally required. Regulation by two- or three-heat switches or by thermostatic control will therefore be necessary if continual switching on and off is to be avoided and any approach to an even temperature secured. In this connection it will be recalled that a steam radiator always has adjacent to it a regulating valve by which the inlet of steam can be adjusted to a very fine degree.

The situation is then that in replacing steam with electricity for shipboard heating the designers must aim at a high degree of perfection. It is hardly possible to equal steam at its best without the use of a thermostat, and, most important of all, the electric heater must eventually embody that very valuable property of a steam radiator of never giving rise to fire risk. Fire risk is a danger which has to be watched with particular care in ships' equipment, for in addition to the difficulties in dealing with fire at sea, which are very great indeed, and the dangers of escape from it, there is the serious factor of the very rapid spread of fire, as a result of most of the inside construction of the ship being of combustible materials.

RULES AND REGULATIONS IN RESPECT OF ELECTRIC HEATERS

There are somewhat stringent rules regulating the design and installation of electric heaters, laid down by various bodies controlling the construction of ships, such as the Board of Trade, Lloyd's, and the British Corporation, and in Table 3 the principal requirements are co-ordinated.

The most important technical point in this somewhat formidable list is the restriction on radiant heat and insistence upon convection heating. Lloyd's rules dealing with this point are recorded under a paragraph headed "Air Heaters" and commence by saying "The construction of heaters is to be such as to heat the surrounding air by convection," while the British Corporation clinch matters by saying "Types in which heating is obtained by direct radiation from the elements may only be used in public rooms." There can be no doubt that for marine purposes heating by radiation would be better than by convection, particularly from the point of view of comfort. The principal object of heating a ship is to provide comfort and conditions conducive to the health of passengers and crew on board, other requirements being amply secured if the comfort conditions are properly filled. It seems unfortunate, therefore, that the use of radiant heat should be debarred as such rather than merely restricted with suitable safety conditions.

RADIANT VERSUS CONVECTION HEATING

Every body at temperatures above absolute zero radiates energy, which when it strikes matter is absorbed to a varying degree and produces heat. We term such radiation "radiant heat." In respect both to wavelength and intensity it varies with the temperature of the radiating body, and while for a heating appliance the

Table 3

THE PRINCIPAL RULES AND REGULATIONS RELATING TO THE CONSTRUCTION AND INSTALLATION OF ELECTRIC HEATERS ON SHIPS

Item	Authorities
Heaters are to be durable and all parts are to be of strong construction	Lloyd's
Construction to be such as to heat the air by convection	Lloyd's
Types in which heating is obtained by direct radiation from the elements may only be used in public rooms	British Corporation
Heaters in which the elements glow or reach a temperature which will ignite explosive vapours or dust in surrounding atmosphere are not to be used except where fitted in suitable situations in public rooms	Lloyd's
Portable appliances are to be of such shape and so weighted that they cannot be easily overturned . . . to have suitable stowage position . . . a suitable clip or bracket is to be fitted for holding the heater in a fixed position	Lloyd's
Shall be so constructed (I.E.E. and Lloyd's and British Corporation) and mounted (I.E.E. and Lloyd's) that their supports and those parts which have necessarily to be handled in their operation cannot . . . (I.E.E. and Lloyd's) that no part which has to be handled by the operator shall . . . (British Corporation) become heated above 130° F. (54° C.)	
Where installed on a deck or bulkhead to be so mounted that there will be at least 2 in. air space between the heater and bulkhead They are to have non-inflammable heat-resisting material between the heater and the surface on which they are mounted and to which they are adjacent	Lloyd's
Are to be placed so as to diffuse heat adequately and assist ventilation and must not be so near to the heads of beds in sleeping rooms or seats in mess rooms as to cause danger or discomfort to occupants	B.O.T. (master and crew spaces)
All inflammable materials in the vicinity of heating appliances where fitted in the ship are to be ("effectively," British Corporation) protected by suitable fire-resisting materials	Lloyd's and British Corporation
High-temperature parts of radiators shall be suitably guarded	I.E.E. and Lloyd's
Must be properly shielded as a precaution against risk of fire and danger to the persons using the space	B.O.T. (master and crew spaces)

In addition to the above there are a number of requirements regarding details such as the elements and the guarding and connections to them, also as to the fusing and switching, none of which is relevant to the general subject of this paper.

surface temperature must be reasonably high in order to secure quantity within suitable dimensions, penetration, and throw, at the same time radiation occurs at all temperatures.

As radiant heat can be reflected in the same way as other radiation, we can arrange matters to give a beam of warmth which will penetrate the air and which in striking a person will also penetrate his clothing so that its warming effect on the clothed body is felt almost immediately.

Heating by convection is totally different; it is a process of air heating whereby we surround ourselves with a blanket of warm air. It may take some time to make the blanket, and a person who is thoroughly cold when coming into it, destroys it locally to some extent while he is heating up and he will only feel the effect of the heat

on those parts of him that are bare. So far as his clothed body is concerned he will only feel it in so far as his breathing expels the cold air out of his clothing and replaces it with warm air, and in actual fact the heat of the air does not fully reach his body until the temperature of the various layers between him and the atmosphere has been raised.

We arrive then at the conclusion that with convection heating his body actually heats itself, on coming in from the cold outside, by its own emission rather than by the heat in the surrounding air; it will be understood how it is that a man who comes in from a 4-hour watch in an exposed position under winter conditions needs radiant heat rather than convection heat and why it is said amongst sailors that the only way to get warm from an electric heater is to sit on it (heating by conduction).

The use of radiant heat on board ship would solve a number of heating difficulties. By directing emission properly it could, for instance, defeat the severe effects of cold metal surfaces. The Board of Trade are now insisting upon heating for crews' washplaces and bathrooms (a luxury not usually given to the passengers), and as these spaces are usually all bare steel except for the tiled or cement floor the use of radiant heat is almost essential for satisfactorily dealing with this problem within reasonable heat quantities. Again, in heating individual rooms in which there is an open space between the top of the framing and the deck overhead, which frequently occurs in crews' accommodation and in passengers' rooms other than 1st-class, if convection heating is used the rising warm air escapes through these openings and consequently the room rises in temperature at the same rate only and to the same degree as the whole space of which it is an individual part. This effect will be seriously felt if only a part of the accommodation is in occupation. Radiant heat would effectively deal with that situation and would also reduce the ventilation losses.

FIRE RISK, AND CERTAIN DIFFICULTIES IN THE USE OF RADIANT HEAT

While this impressive case can be made for the use of radiant heat, in practice there are difficulties in its application which have not yet been satisfactorily solved. First there is the question of safe temperature. There is, of course, nothing so effective as a luminous source. At the same time it is not essential to have the radiating body at a dangerous temperature, and in the ordinary way there is very little fire risk below 600° F. But the danger lies not so much in the working temperature as in the possibility of overheating to a dangerous degree.

It will be realized that there is nothing in an electric heater—whether it be of the radiant or the convection type—to prevent it rising to a dangerous temperature if for any reason it cannot get rid of the heat as quickly as it produces it. When such a condition occurs it will naturally go on increasing in temperature until either a balance of input and output is reached or something occurs to stop the flow of current. This is well illustrated in a case of fire with a convection heater which had all the appearance of absolute safety. It was recessed into wood panelling, but was so constructed that by ample circulation of the air the outside of its metal body did not rise in temperature to as much as that of boiling water. A heavily upholstered chair with a holland cover was pushed right up against it so that the air circulation was almost completely stopped, and the heater then being very excellently insulated on all sides became in effect an oven in which the temperature continued to rise through a period of 36 hours until fire occurred. Now, unfortunately on board ship people —the crew as much as the passengers—do amazingly foolish and dangerous things, things which they would never think of doing in their own homes, and it is impossible to prevent them hanging clothes and towels, rugs, stockings, and flimsy, highly inflammable garments, over heaters or too close to them, with consequent fire risk. At the moment there are very few heaters on the market free from fire risk if garments are thrown over them.

The fact of the matter is that the electric heater as it exists to-day is too simple an apparatus. It converts electricity into heat with almost 100 % efficiency and without restriction, and it needs to be harnessed. Our aim should be, however, not to harness it by means of a number of restrictive devices which from the marine point of view would be undesirable, but by some construction which would introduce natural inherent limitation. An element is needed with a temperature/current characteristic such that it will not rise in temperature above a certain degree; that does not seem to be a very impossible thing to demand, certainly not one beyond the skill and ingenuity of the electrical engineer and metallurgist of to-day.

Until something of the kind is produced there seems little hope of designing a radiant heater safe enough for the general heating of ships, but one hesitates to say that radiant heaters should not be used at all.

Panel heating has of course been suggested, but although it might be possible to use it in public rooms in some cases it would be almost impossible to use it in cabins for either passengers or crew, principally because of the difficulties which arise in relation to furniture and fixings. The cost of installing would also be prohibitive.

The difficulty of securing a good position for the heating units is the second difficulty in the use of radiant heat, for it is important that the beam should be properly directed. It is almost certain that the design would be for fixing overhead, and limited headroom, appearance, and difficulties of avoiding interference with the lighting units make it by no means easy to find good places for such radiant heaters in passenger cabins. In alleyways, particularly in passenger accommodation, it would be almost impossible to use them. Actually it would be easier to find good positions in the officers' and crew's rooms than anywhere else, and as it is in that accommodation that radiant heat is most needed and it is always difficult to find good positions for the heaters on the bulkheads in crews' rooms, the possibility of radiant overhead heating is attractive.

CONVECTION HEATERS

It hardly seems necessary to say anything nowadays about convection heaters, yet it is only a few years ago that the designs were so unsatisfactory from the marine point of view that the shipbuilding firm with whom the author is associated was forced to design one for ships' use. The three principal desiderata which were laid

down in working out that design have now become embodied in its many competitors, and much ingenuity has been shown in securing them. They are:—

(1) Absence of fire risk, even when clothing and towels are thrown over them.
(2) A surface temperature low enough to give anyone (particularly children) warning that it is hot before contact actually burns a delicate hand or the face of a child who falls against it.
(3) A construction such that it will induce circulation of air through it strongly, heating the air adequately in the process without overheating it.

In addition, the psychological importance of making a heater look hot was dealt with by illuminating metal bars in such a way that they appeared to be red hot.

Fire tests both with very light material such as voile, and with heavy material such as a heavy bath towel, are required, and the tests should be taken with the heater fixed to a bulkhead so as to simulate accurately the conditions on board ship.

The surface temperature is of course merely a matter of design and the air temperature also, since the final temperature is a measure of the amount of air passing through it. These should both lie between 150° F. and 200° F., the latter being just too hot.

In the author's opinion convection heaters should never be recessed. It is sometimes stated that recessed heaters are perfectly safe if surrounded with asbestos, and possibly they would be if the surrounding heat insulation were $1\frac{1}{2}$–3 in. thick, but such heavy insulation is never used, and, bearing in mind the possibility of stopping or seriously throttling air circulation, it is far better to provide plenty of surface over which the heat can be readily emitted.

An important effect of convection heat in relation to ships' heating is the lowering of the degree of humidity of the air. The humidity at sea is naturally high, and for the best conditions of warmth-comfort it should be between 40 % and 70 % at 60° F.

Table 4

SAVING DUE TO THE USE OF THERMOSTATIC CONTROL OF ELECTRIC HEATERS, AND THE TIME (ACTUAL) IN WHICH THE COST OF THE THERMOSTAT WILL BE RECOVERED

Heater size, kW	Saving in £ effected per year per heater with energy at $\frac{1}{2}$d. per unit and with total use and total saving of energy as shown			
	Total use 3 months in the year		Total use 6 months in the year	
	Thermostat saving, 10 % units	Thermostat saving, 20 % units	Thermostat saving, 10 % units	Thermostat saving, 20 % units
0·75	0·34	0·68	0·68	1·37
1	0·46	0·92	0·92	1·84
2	0·92	1·84	1·84	3·65
3	1·37	2·74	2·74	5·48

Heater size, kW	Thermostat would save its cost in years of life as shown below:—			
	Total use 3 months in the year		Total use 6 months in the year	
	Thermostat saving, 10 % units	Thermostat saving, 20 % units	Thermostat saving, 10 % units	Thermostat saving, 20 % units
0·75	7·35 years	3·7 years	3·7 years	1·8 years
1	5·45 years	2·7 years	2·7 years	1·35 years
2	2·7 years	1·35 years	1·35 years	8 months
3	1·8 years	11 months	11 months	5·5 months

TEMPERATURE CONTROL BY THERMOSTATS

Apart from the fact that our aim in designing the heating installation should be as much towards perfection as we can practically attain, the factors we have to deal with on board ship make both a particularly strong case for automatic control and an equally strong case against leaving control wholly to the hazard of adjustment by the people on board. The following summarizes the situation:—

(1) In addition to the fact that weather conditions may alter and fluctuate during 24 hours, the ship will travel 500 miles or more during that time, and that may give rise to appreciable variation. We have, therefore, to provide for a particularly

unstable state of weather and temperature conditions.

(2) As the effect of the sun's rays on the steel ship's sides is very quickly and appreciably felt, rooms with substantial wall area exposed to the weather and sun will have a widely varying heat loss on sunny cold days, and as, further, outside conditions affect different parts of the ship in widely different degrees, there is on large ships a good deal of individual adjustment necessary which thermostatic control will deal with better than other methods.

(3) On passenger vessels personal taste and opinion as to temperature must be catered for, and we have to bear in mind that many passengers, particularly ladies, dislike touching electrical equipment, even ordinary switches, with which they are unfamiliar. The less passengers have to touch equipment and manipulate switches the better, both from the point of view of upkeep and to avoid frivolous claims for damages.

(4) From the point of view of economy, automatic adjustment is desirable. In addition to saving units it also assures that the kW demand is kept to the minimum, and this may save running a generating set unnecessarily.

(5) As a measure of safety thermostatic adjustment is particularly desirable to prevent overheating, especially in small rooms with walls of wood or pulpboard, and a covered deck and possibly a panelled ceiling overhead. In such rooms with ventilation shut off and a heater left full on there is a grave risk of overheating, with consequent damage or even fire.

Actually thermostatic control can save its cost in a very short time, as can be seen from Table 4.

Thermostats for ships' use differ in detail from those for shore installation, the principal difference being that they are almost always operated on direct instead of alternating current. A specification for suitable instruments is included in the Appendix.

TESTS ON BOARD NEW SHIPS

It has not been customary in the past to test marine heating installations beyond going round the accommodation to make sure that it was comfortably warm, but now that the Board of Trade has specified a temperature-rise, temperature tests are called for in the masters' and crews' spaces on all new ships.

The position at present in regard to these tests is not very satisfactory, in that the conditions of test are not defined. Further, as they are taken to confirm that requisite service conditions have been secured, the conditions during test should be such as to provide a reliable indication of what the heating will be under service conditions, and that is almost impossible to attain. Bearing in mind the effects of trade route, also the fact that a new ship like a new building takes a considerable time to heat up and dry out, it will be realized that tests on a new ship may require very much more heat to reach and maintain a stated temperature than will be necessary in service. It would be better if such tests could be taken after the ship had made two or more representative voyages, but that again presents difficulties.

It is interesting to note, in this connection, the requirements of the Institution of Heating and Ventilating Engineers for temperature tests on the heating of buildings. These are that the entire installation shall have been in continuous use for not less than one calendar month immediately previous to the test, that the building must be dry, that normal atmospheric conditions prevail, and that the doors, windows, and ventilators, must have been kept closed for 24 hours before the test and must remain closed during the period of test.

Then there is the question of temperature. It has been amply established that air temperature is not a true measurement or indication of warmth. A joint committee of the Medical Research Department and the Department of Scientific and Industrial Research investigated this question some 7 years ago and upon the basis that "if the term warmth is invested with a scientific meaning it should connote the proper assessment of all thermal factors, including humidity, which induce human comfort" they examined the various instruments and methods existing for the exact measurement of warmth. It had been established that such measurement should be on the basis of the heat-loss rate of a sizeable body kept at the same skin temperature as the temperature of the outside surface of a clothed man, and they reached the conclusion that the eupatheoscope designed by the Building Research Station might be regarded as the standard instrument for ordinary and usual conditions. Unfortunately that instrument is rather cumbersome for practical tests on board ship. Referring again to the Institution of Heating and Ventilating Engineers' recommendations, we find that they lay down a scale of temperatures thus:—

"If the external temperature at the time of the test has been or is above or below 30° F. and is not rising at a rate greater than 1 deg. F. per hour, the following shall be considered the equivalent relative temperatures (°F.) to 60° F. inside when the external temperature is 30° F.

External	25	30	35	40	45
Internal	57	60	62	65	67."

It might be said that the best way out of the difficulty is to install such ample heating that temperature tests would be obviously unnecessary, but as surplus heat is more than a matter of the cost of a few more watts in the heater, affecting as it does the whole distribution system back to the main switchboard circuit-breakers, the remedy is hardly an acceptable one. It has been shown that on cold-weather ships far more heat than is necessary to give the temperature-rise from 30° to 60° F. may fail to give adequate heating. Experience also shows that some ships may be adequately heated in service with a good deal less heat than is required to give the B.O.T. temperature-rise before they have received the drying-out and heating-up that they will normally and constantly receive in service as a result of spending most of their time under tropical sun.

We reach the conclusion, then, that the old rule that "proper and adequate" heating should be provided was wisely framed and worded and that everything points to tests by thermograph charts in service, but for the diffi-

culties of securing that the controlling conditions have been properly looked after during test. It is unnecessary to detail those conditions, but four points of importance which have been found to give rise to widely different results in the same compartment of the same ship with the same amount of heat switched on may be mentioned:—

(1) The sensitivity to change of temperature, to sunshine, and to wind, of bare steel ships' sides and bulkheads exposed to the weather.
(2) The necessity of making sure that the heating is full on in the whole compartment, also sometimes in those above and below and fore and aft of that compartment.
(3) The advisability of shutting off all ventilation during test, making suitable allowances for air changes afterwards.
(4) The considerable "soaking" time necessary on new ships.

CONCLUDING REMARKS—COSTS AND CIRCUITS

The capital cost of electric heating for ships is usually less than that of other systems, for it is cheaper to run cables than pipes and trunking and it is often possible to get mains capacity almost without cost by taking advantage of the capacity necessary for what is termed "hot-weather load," namely excess load on cooling-water pumps, fans, and refrigerating load, etc., the demand on which drops with the temperature of air and sea. As a result also of this drop in the load with cooler conditions, extra generator capacity is not as a rule required for electric heating load.

In running cost also electric heating is cheap, for as it is a demand-leveller it is seldom necessary to run extra generators and the cost of the units is therefore no more than the cost of the extra fuel and lubricating oil used. As a matter of fact electric heating is sometimes a welcome makeweight when the hot-weather load disappears from Diesel generators, for otherwise the engines may be running at too light a load.

Further, in upkeep costs electric heating is cheap, for the upkeep of cables is negligible and, as the elements work at black heat, renewals are few and far between. Recently data of a ship with an extensive electric heating equipment which had been in service for 5 years showed less than 1 % of element renewal in that time, and the electrician reported that he did not remember any breakdown or overhaul or repair other than the few elements.

One point in relation to cost may, however, be mentioned, and that is the important one that heater subcircuits are far too costly. Circuits to individual small items on board ship are always costly. Sometimes they are of considerable length and require, for the cables, individual supporting arrangements, such as perforated plating or specially drilled and tapped fixing holes for each clip, and such a circuit may easily cost over £5. By the rules, every heater of over 660 watts at 220 volts and 300 watts at 100 volts must have a separate circuit, and every heater of over 1 320 watts at 220 volts and 600 watts at 100 volts must have a switch as well as double-pole fuses in its fuse-box. Incidentally, it is exceedingly difficult to find suitable places for these large switch and fuse-boxes. If a new class of sub-circuit—namely "final sub-circuits for heating"—were made on the same lines as the existing final sub-circuits, but with the capacity of a 7/·029 vulcanized-rubber cable (18 amp.), using distributing boxes with fuses only, the switches being installed either at the heater or the fuse as desired, it would overcome a number of difficulties without giving rise to any kind of risk. Three or four heaters could then be looped on the one circuit, thus encouraging an extended and desirable distribution of heat instead of concentration in large units.

Experience has shown (R.M.S. "Olympic," built nearly 30 years ago, had 750 kW of electric heating installed, while other installations of equal size have been in service 10 and 15 years) that such circuits would be absolutely sound electrically, for there is far less trouble on heating circuits than there is on lighting circuits, all parts being more robust and the various clearances being considerably greater.

In conclusion it may be said that the demand for electric heating for merchant ships is strong; the use of electricity for ships' heating is increasing and everything points to growth in its use, provided that development and improvements are secured to meet the special needs and conditions of marine service.

APPENDIX

Calculation of Heating—a Check from Test Figures

Example.—Small passenger vessel for British coastal trade.

Data of crew's room (12 *men*):—

(*a*) Forward bulkhead, bare steel, cargo hold other side, area = 114 sq. ft.
(*b*) Aft bulkhead, bare steel, chain locker other side, area = 168 sq. ft.
(*c*) Ship's sides, P. & S. bare steel = 224 sq. ft.
(*d*) Ceiling overhead bare steel, seamen's room over = 289 sq. ft.
(*e*) Floor, patent decking on bare steel, cargo below = 118 sq. ft.
(*f*) Volume = 1 620 cu. ft.

Test 1.

Taken in port in fine calm weather. Outside temperature varied during the 24 hours from 35° F. to 53° F. Heating = 2 kW, i.e. 1·23 watts per cu. ft. Temp.-rise = 12 deg. F.

Calculated heat losses.—Under the calm conditions prevailing, the loss through bare steel is taken at 1·0 B.Th.U. per sq. ft. per deg. F. difference per hour = 6 805 B.Th.U. per hour.

Ventilation losses may be taken as negligible per hour, as there was no wind and natural ventilation was fitted.

Actual losses = 2 kW = 6 830 B.Th.U. per hour.

Test 2.

Ship's sides lined with pulp-board $\frac{3}{8}$ in. thick and the test repeated with 3·2 kW of heating. Weather conditions as before. Temperature outside varied from 36° to 46° F. Temp.-rise = 24 deg. F.

Ventilation losses could again be regarded as negligible, but this was tested by blocking the ventilator halfway through the test and was shown to be a correct assumption.

Calculated losses.—Taking transmission factor for panelled ship's side as $0 \cdot 52 = 11\,050$ B.Th.U. per hour.
Actual losses $= 3 \cdot 2$ kW $= 10\,920$ B.Th.U. per hour.

Conclusions.

Satisfactory heating for this room to secure 30 deg. F. temperature-rise with ship's side panelled, and allowing 3 changes of ventilating air per hour and 12 occupants, would be:—

(a) & (b) $= 282 \times 1 \cdot 0 \times 30 = 8\,500$ B.Th.U./hr.
(c) $= 224 \times 0 \cdot 52 \times 30 = 3\,490$ B.Th.U. ,,
(d) Nil
(e) $= 118 \times 0 \cdot 52 \times 30 = 1\,840$ B.Th.U. ,,
Ventilation $= \dfrac{1\,620}{53} \times 30 \times 3 = 2\,800$ B.Th.U. ,,

Total $= 16\,630$ B.Th.U./hr.

Deduct 12 men at 350 B.Th.U. per hour $= 4\,200$ B.Th.U. ,,

Heat to be provided $= 12\,430$ B.Th.U. per hour $= 3 \cdot 65$ kW

Actually 5 kW was fitted in the room; this is equivalent to $3 \cdot 1$ watts per cu. ft.

Specification for the Construction of Thermostats for the Control of the Heating on Merchant Ships

General design.

Artistic appearance is important and must be such that any part fitted in a public room or passenger's cabin is unobtrusive. The apparatus in crew spaces must be very robust and preferably contained in a strong metal housing.

The possibility of unskilled interference with the units is important, and designs in which adjustments are only possible by skilled manipulation are preferable.

Instruments in which the sensitive element only is fitted inside the room, with the actuating instrument outside connected either by wires or by capillary tubes, would sometimes be advantageous.

No parts that rust or are otherwise attacked by sea air are permissible; the instrument throughout must be able to withstand humid conditions without deterioration. Plastic mouldings must be of a quality such that they will not "track" as a result of exposure to salt air.

The possibility of the instruments being under the direct action of sea air must be visualized. Instruments which require frequent opening-up for cleaning, or which are so constructed that the inside would become dusty, would not give good service on board ship. The construction must also be such that if the units are interfered with by children no harm will come to them.

All moving parts, and in particular bearings and pivots, must be designed generously. It must be borne in mind that when in service the units will be in use continually night and day.

Table 5

CALCULATION OF HEATING IN THE BO'SUN'S ROOM MENTIONED ON PAGE 422 AS BEING INADEQUATELY HEATED UNDER VERY SEVERE CONDITIONS

Data.—Dimensions 8 ft. × 8 ft. × 7 ft. high.
Exposed position aft. Two sides exposed to weather, also the deck overhead.
Volume (disregarding furnishings) 450 cu. ft.

Item	Area, sq. ft.	B.Th.U. loss per hour under the conditions shown, at inside temperature 60° F.		
		Mild conditions in port. Outside temp. 30° F.	At sea in winter. Outside temp. 25° F.	Severe cold and weather. Outside temp. 15° F.
Ship's side bulkhead. Bare steel	42	1 260	2 940	4 730
Area covered by furnishings	14	168	216	290
Bulkhead to well deck. Bare steel	44	1 320	3 080	4 950
Covered by furnishings	20	240	308	414
Bulkhead to passage. Bare steel	22	440	550	770
Covered by furnishings and door	34	272	374	548
Bulkhead to stairs. Bare steel	30	450	600	900
Covered by furnishings	34	204	299	450
Deck overhead. Bare steel with 2½ in. wood	64	922	1 165	1 558
Floor. Accommodation at same temp. below	64	Nil	Nil	Nil
Ventilation, 3 changes per hour		260	300	390
Total B.Th.U. loss per hour		5 536	9 832	15 000
Equivalent kW		$1 \cdot 63$	$2 \cdot 88$	$4 \cdot 4$
Size (kW) of heater to be fitted		$2 \cdot 0$	$3 \cdot 0$	$4 \cdot 5$
This gives watts per cu. ft.		$4 \cdot 45$	$6 \cdot 65$	$10 \cdot 0$

Robustness.

The whole instrument and all parts in connection with it must be strong enough to withstand the abuse experienced in ship's service.

Sensitivity.

A range of 6 or 8 deg. F., that is 3 deg. or 4 deg. on each side of the setting, is sufficient. Extreme sensitivity is undesirable from the point of view of upkeep.

Temperature adjustment and temperature range.

A large range of temperature adjustment is unnecessary, the only adjustment required being such as to rectify the setting to suit local conditions. The means of adjustment should be enclosed inside the instrument and may be arranged to be operated by a screwdriver after removal of the cover. Provision of any outside knob or means of adjustment is undesirable, and no showing of the setting is required. The whole arrangement should be such that interference by passengers, stewards, and crew, is difficult. Adjustment between 55° F. and 80° F. would be ample, and an indication of degrees on the adjustment is unnecessary.

Current rating, switch connections, and contacts.

Sizes to control heaters of 0·75, 1, 1·5, 2, and 3 kW. Direct current is usually employed. The whole equipment must be silent in action. Contacts must be both positive and well held, and such that they will not chatter or spark under the vibration experienced when the ship reverses and in bad weather.

Addendum.

The choice of the position on board is often difficult, and the following must be borne in mind in this connection:—

(1) Adjacent bare steel, whether in the form of beams, decks, or bulkheads, gives false readings. The instruments must never be fixed against bare metal.

(2) Incoming ventilating air blowing on the instrument, both directly and by way of deflection, must be avoided; also draughts from portholes, which not only lead to false action but at times carry salt spray. Draughts from louvres in doors, or from open spaces between the top of the framing and the deck overhead, also lead to false action.

DISCUSSION BEFORE THE INSTITUTION, 24TH NOVEMBER, 1938

Mr. G. O. Watson: The author rightly points out that it is no kindness to overheat the crew's accommodation, and probably the Board of Trade had this in mind when they fixed on a temperature of 60° F., which is 4 deg. F. below the ideal for human comfort. It has been stated by Thomas Chester that if one could draw a comfort curve it would be a parabola, the ideal air condition being at its apex. Temperatures of 3 deg. F. below and 3 deg. F. above the optimum comfort value represent comfortably cool and comfortably warm conditions respectively, 6 deg. F. below or above is either too cool or too warm, and 9 deg. F. is definitely hot or definitely cold; so that we have a comparatively small range on which to work.

It is apparent from the paper that owing to changing conditions the problem of electric heating for merchant ships is more complex than that of electric heating of buildings. It is obviously unsafe to arrange the heating installation purely to suit the cargo or the trade route for which the vessel is built. A ship may and often does change hands even while it is still building. Even vessels which trade normally from English waters through the tropics may spend 2–3 weeks in dock between voyages. In one such case I had occasion a few years back to investigate maximum-load conditions, and found from the logs that the peak had occurred one bleak November when the vessel had left dock with a complement of passengers and was fogbound in the Thames for 3 days. The peak was due principally to lighting and heating.

Coming now to the latter part of the paper, wherein the author discusses types of heater, one's first impression is that he has a grievance against Lloyd's for its restrictions on the radiant type of heater, but as one reads on one finds references to fire risk and that " there are difficulties in its application which have not yet been satisfactorily solved." The Committee of Lloyd's Register of Shipping is always prepared to grant permission for a departure from its Rules where it is satisfied that the proposal is a safe one and equivalent to the standard indicated in the Rules; so that any shipbuilder or supplier who comes forward with a proposition embodying radiant elements which he can demonstrate is as safe as a convector-type heater, will meet with no opposition.

An example is quoted where the outlet of a convector-type heater was blocked and the temperature rose steadily for 36 hours until fire occurred. This presupposes that (*a*) the chair was of just the right height and shape and was so placed that it completely blocked the opening; (*b*) no one noticed the absence of any heating effect from the heater; and (*c*) during 36 hours no one noticed any smell of paint or burning. Laboratory tests with certain types of convection heaters have shown that restricting the free circulation of air causes the heating element to burn out before any damage is done.

Protection can be given in two ways. One is by a method I came across in Belgium, where a fuse was placed inside the framework in such a position that it blew if the temperature of the heater rose considerably above its normal value. The other alternative is to place a thermostat on the inlet side. This serves two purposes: (*a*) The air in the room re-circulates through the heater, and when it reaches the desired limit the thermostat comes into action. (*b*) If the outlet is shut off, the air within the case re-circulates internally and has the same effect. The former method is not so satisfactory in crew's quarters,

because the heater frame has to be dismantled to enable one to renew the fuse, and after it has operated once someone will see to it that he does not have to replace the fuse a second time. The thermostat will reset after a time, and in uninformed quarters its presence may even be unsuspected.

The ideal method of heating, apart from air conditioning, is undoubtedly a combination of radiation and convection, but investigations into fires on board ship have shown that many so-called electrical fires have started from radiators. Blankets and garments left to air or to dry have fallen against the elements, with the inevitable consequences. It is interesting to observe that in Canada there are so many houses constructed of wood that the regulations relating to radiant-type fires are so stringent as almost to preclude their use.

Radiators fitted overhead, as the author suggests, might be one solution, but it would be necessary to guard against broken elements, such as the rod type used in reflector-type heaters, falling on to carpets and furnishings or on to the occupants of cabins.

His remarks on steam radiators rather suggest a possible solution, at any rate so far as the crew's quarters are concerned. Why not start off with a hot-water radiator of this shape, and fit an immersion heater and a thermostat? Most electric radiators are of small size and run at a high temperature. A steam or hot-water radiator has a large radiating surface and works at a comparatively low temperature. The thermostat could cut out below boiling point and a safety plug or valve could be fitted to give protection against excessive internal pressure. The addition of a fan to convector-type heaters to improve the rate of circulation would also achieve the effect of projecting hot air on to the body and clothing and so hastening the "warming up" process when a seaman comes in cold after his 4 hours' watch.

I have often considered whether it would be a good thing to insist on a pilot lamp to indicate whether a convector-type heater is "on." The hot grill of a convection heater is a harmless-looking object, often placed about 3 ft. from the ground, convenient to the hands or the face of a child and hot enough to burn the skin. A pilot lamp, suitably placed, might also be used for "effect" purposes.

The Tenth Edition of the I.E.E. Wiring Regulations informs us that a pilot lamp is under certain prescribed conditions not deemed for the purpose of the Regulations to be a final sub-circuit—in other words, it need not be separately fused. In a certain type of heater which I examined recently the lamp could be switched on without the heater, and vice versa. Such a lamp is obviously not a pilot lamp, and according to the Regulations should be on a separate circuit. One usually associates pilot lamps with switch-boards and ironclad cooker control units, etc., but in this particular application the lamp and reflector were attached to a wood panel, the wiring being cleated behind the panel, and the only fuse protection being that of the heater circuit.

As stated by the author, Lloyd's rules at present require 220-volt heaters of 660 watts and over to be on separate circuits, and those of 1 320 watts and over to have a switch at the fuse-box; but it is hoped to relax this requirement before very long.

Mr. J. W. Kempster: It will be noticed that the smallest coefficient given in Table 1 is 0·25, and the highest coefficient is 2·5, 10 times as great, which illustrates what an extraordinary range of heat loss has to be dealt with on board ship. It would be informative to know to what extent this Table is applicable to other ships, and what is the accuracy of the various figures.

I cannot accept the author's statement that a ship has nothing of a seasonal nature about it such as is associated with shore heating. For instance, in the case of a ship going from New York to Southampton and back there is both a seasonal variation (because it is much colder on board in winter than in summer on both sides of the Atlantic), and also a climatic difference (because in New York it may be much colder or hotter than in this country at the same time of year). On a voyage to the Antipodes no doubt climatic conditions are more important than seasonal, but in the same latitude the seasonal factor is often quite as important as the climatic.

The heating of a ship is bound up with its ventilation. A figure of 3 air-changes per hour for passengers and crew is too low for comfort. I take it, however, that 10 to 15 changes per hour is what the system mentioned in the paper is designed for, but that in practice one passenger turns off his louvre or his ventilator and another does not, so that so far as a whole compartment is concerned a kind of load factor enters into the problem which reduces the average figure to 3 air-changes per hour. This seems low, but has the author assumed that in ventilating the ship cold air is put in, and not warm air, which in cold weather is desirable?

In Table 4 he refers to the saving due to the use of thermostatic control. I consider that his figures for the saving are a conservative estimate, for the reason that in default of thermostatic control the passenger naturally turns his cabin heater on when he wants heat, and presently goes out of the room. When, after some time, he comes back, he probably finds the cabin far too hot, and he then turns the heater off, many heat units having been wasted in the interval. But with thermostatic control the heat generated may be regulated to a nicety, with consequent economy.

No doubt electric heating is to some extent a demand leveller, but unless the so-termed "hot weather load" is relatively appreciable, the maximum-demand load will prevent the levelling effect from being very considerable.

The author says that heating by warm air is expensive, but surely it is less expensive than heating by electricity. With electricity at, say, $\frac{1}{3}$d. a unit one obtains about 10 000 B.Th.U. for 1d., whereas with steam generated from coal, even allowing for considerable losses, one obtains about 25 000 B.Th.U. for 1d. If it could be assumed that it was not necessary to ventilate the ship the problem would be different, but any first-class passenger ship has ventilating trunks, and it costs very little more to provide warm air instead of cold air so far as the trunk system is concerned.

A very good point is made in the paper with regard to the effect of lining. Wood lining is a bugbear, as it is costly, harbours dirt and vermin, adds to the fire risk, takes up a good deal of space, is heavy, and is not necessarily artistic. Personally I prefer accommodation of bare steel, cork-dusted.

I agree that air temperature is not an adequate criterion of heating efficiency from a human standpoint. Certainly for comfort the air must be kept on the move, and also have a proper degree of humidity. I suggest that a basic range of temperature from 30° to 60° F. should be produced by warm, and preferably conditioned, air, and that the balance (below 30° F. or above 60° F.) should be supplied by the electric heating. There should be a thermostat on each heater, with provision for switching the thermostat off.

With regard to the choice of heater, I consider that the radiant type is by far the best, despite the fire risk associated with it, though some types of electric radiant heaters are quite safe. By throwing a towel over a non-radiant heater one is just as likely to start a fire as with the usual radiant type; the only difference is that with the radiant type the fire is likely to start earlier.

Mr. M. I. Lipman: I fully agree with the author's emphasis on the necessity for thermal insulation, which is just as important on the ship's side as on the boiler. Insulation pays for itself very rapidly. In the case of shore heating, with heating hours of 1 500 to 2 000 per season, very expensive insulation of the cork type, even when accompanied by metal foil, will pay for itself in two seasons. Where heating hours are longer, as is usually the case on board ship, insulation will pay for itself in an even shorter period.

With regard to the question of blown hot air versus local heating, hitherto the tendency has been to adopt the former system, in spite of the necessity of thermally insulating the ducting. It is increasingly becoming the practice to supply the heat locally in conjunction with blown air at a lower temperature, suitable heaters being installed at the terminal point of the ducting.

Turning to the vexed question of electric radiant versus convection heating, some recent experiments with which I have been associated have given extremely interesting data on the question of comparative heat loading per cubic foot of space or per square foot of wall area for the two methods. Radiant systems invariably show a greater consumption for the maintenance of a given air temperature than convection systems, firstly because they have no direct heating effect on the air, the eventual air-heating resulting from the heating-up of intermediate objects. Secondly, unless the radiators be so placed that they do not project any radiation on to uninsulated outside walls, the heat lost by the direct impingement of radiation on the wall is far greater than that lost by conduction from warm air through uninsulated bulkheads and wall surfaces. These experiments further emphasize that whatever the system, there is everything to be gained by greater use of insulation.

The author mentions the inadvisability of building-in convector heaters. It is obviously dangerous to hide behind panelling, or inside a piece of furniture, a convector the casing of which becomes unduly hot. There is now, however, a convector available of a type which does not suffer from this objection; it consists of an element surrounded by three successive streams of air. Each of these streams is the air-cooling jacket for the previous one, and the sum total comes out at the top as a fairly high-speed jet of convected air. The outer jacket is so cool that the surface temperature-rise on the case is not more than 2 or 3 deg. F. Convectors of this type have been employed with considerable success built into wardrobes and washstands in cabins. A thermostat can be built-in near the air inlet, which will switch off the heater should the temperature inside build up to a dangerous level as a result of the air flow being obstructed.

A popular means of increasing the flexibility of heating systems at very low capital cost is to provide on board a small stock of convectors of the portable type, which can be connected to a suitable plug in the cabin. Passengers' individual tastes in temperature can thus easily be accommodated. Such portable heaters can also be controlled by built-in thermostats, which fulfil a safety function as well as serving as a temperature control.

Mr. C. W. Saunders: The heating of ships is a very difficult problem because of the seasonal and climatic changes encountered on each voyage. From the fire-risk point of view, heat is best provided by radiators fitted with immersion heaters, and probably embodying a lamp to give a glow with its associated psychological effect, but these generally are ruled out on the score of cost.

Where air is provided from the deck for ventilation purposes, if steam heating is used, there is always the risk of the valves being frozen up or sticking, and there are great losses in the trunking. The ideal place to provide heating is at the cabin end of the trunking where the air is directional; and if an electric heater under the control of the passenger, without risk of fire, could be provided, this would present the best solution. The switching-on of the heater, of course, must be interlocked somehow with the flow of air. All radiators should be so designed that they cannot set fire to towels, articles of clothing, or even pieces of tissue paper, and they should be capable of being left on indefinitely. It is probably uneconomical from many points of view to have more than 500 watts per cabin.

The subject dealt with in the paper has not been treated very scientifically in the past, and there is plenty of scope in it for the best scientific brains. The co-operation of scientific and practical men in connection with this problem is probably more important than with most other subjects.

[The author's reply to this discussion will be found on page 444.]

NORTHERN IRELAND SUB-CENTRE, AT BELFAST, 15TH NOVEMBER, 1938

Mr. F. H. Whysall: In so far as the complete heating, water heating, and cooking services are concerned, the passenger liner is probably comparable with an hotel or hospital. I had recently to prepare estimates for the complete services for a proposed new sanatorium, and the figures may be of interest. The total loading was approximately 2 800 kVA, the estimated annual consumption 3 million units, and the running cost about £4 250 per annum. The estimated capital cost of the installation amounted to some £14 000, including nine

electrode boilers, thermal-storage tanks, cooking apparatus, etc. It would be interesting if the author could give us any information regarding the recorded consumptions on passenger liners of different types and tonnages. Some information regarding the cost of generation on board ship would be useful, particularly if comparisons could be obtained between the operating costs of geared-turbine and of Diesel-driven generators. It would, of course, be necessary to take into account the capital costs, which in the case of steam sets would include a proportion of the cost of the boiler plant.

Mr. A. N. Savage: With regard to the ventilation losses, I think that if these losses did not occur the atmosphere would become uncomfortable. Where ships are heated by means of warm air, more warm air is delivered than the exhaust fans can extract, with the result that the atmosphere becomes uncomfortable and many complaints of soreness of throat and eyes are received from passengers. With electric heaters, on the other hand, the warm air which is extracted by the exhaust fans is replaced by fresh air from the outside.

With regard to the crew spaces, I would point out that the average crew of a modern liner is in the region of 400 persons. The author states that we should aim at a temperature of 70° F. in the passenger accommodation and that 60° F. is sufficient for the crew's quarters. If his suggestion were adopted, at least 80 % of the crew would be doing manual labour in an atmosphere of 65°–70° F. and spending their leisure time in an atmosphere of 60° F. These men live in very confined spaces, and an epidemic of influenza could quickly develop among them. As no surplus men are carried, it is quite possible that the efficient working of the vessel would be disorganized if many of the crew were laid up. I therefore consider that the heating in crew spaces should be equivalent to that in the passenger accommodation.

While the author makes out a very strong case for radiant heating, I cannot see how any shipowner would be justified in installing radiant heaters, with their attendant risk of fire.

Mr. F. Johnston: The author points out the vast difference between heating a ship and heating a building. First, in the case of a ship one is dealing with a steel structure which is moving through air and water, and these are quickly changing in temperature all the time. Secondly, on a ship one finds passengers of very different characteristics. There may be an American stockbroker in one cabin, and next door a full-blooded English captain. The first feels chilly at 70° F., and the second feels hot when the temperature rises above, say, 60° F. It is difficult to meet such contrasting conditions, but it is necessary to try to do so. In my opinion the only solution is to provide a central heating system, thermostatically controlled, with air conditioning for the complete ship, to give a temperature of 60° F., and to supplement this with electric heating, locally controlled, so that each passenger can turn on the heat to his particular liking.

Mr. S. G. Bittles: It is customary in crews' and immigrants' quarters to install a radiant type of heater which is enclosed in a sheet-steel case. Now these cases are always painted with heat-resisting aluminium paint, a practice which I could never quite understand, since we all know from common experience that for maximum radiation from a warm body, the body should be of a flat black colour. Surely in quarters like those just mentioned the question of appearance alone is not in itself sufficient reason for sacrificing efficiency. I should like to know the author's views on this point.

On page 421 he mentions the " nowadays quite established fact, that the air temperature is not a true measurement of proper and adequate heating." I should be pleased if he would put forward some evidence in support of this statement, and also say what is a true indication of proper and adequate heating.

The author states that the heat loss in still air and normal winter conditions would be 1 B.Th.U. per sq. ft. per hour per deg. F. difference of temperature; presumably this is a fact established from experiment. He does not state for what plate thickness this applies. I should like to know how the thickness of plate affects the actual thermal conductivity.

I think radiant heaters could be more widely employed for heating alleyways. It should be possible to install overhead radiant heaters directing the rays obliquely along the alleyway. I am surprised that such an arrangement has never been tried out, in view of the fact that in order to promote healthy conditions air-changing has to take place continuously. This means, of course, that the heated air is being steadily carried away, and we can see from this that there is necessarily considerable loss with the ordinary convector-type heater. In view of the nature of the radiant heater it cannot be subject to this loss. I should like to know what the author has to say about this point.

Finally, there is the question of individual heater control by means of thermostats. These instruments are rather expensive, and would make the initial cost of the heater installation prohibitive. Is a thermostat employed to control each heater, or does one thermostat control a series of, say, three or four? I should be pleased if some of the outstanding features of the thermostat which the author has in mind could be explained.

Mr. R. J. Shepherd: Public opinion has been responsible for the recent advance not only in the heating of crew spaces but in the welfare of crews generally. The standard of heating in crews' quarters with which we are concerned is the maintenance of a temperature inside of 60° F. when the outside temperature is only 30° F. As a general standard, this is probably acceptable.

The author is very modest in his estimate of the advantages of electrical heaters over other forms of heating. " Bogie " stoves have well-known disadvantages—they are smoky, and expensive to keep up. Steam radiators are used if there are steam boilers on board, but they are liable to become water-locked, and the heat does not always reach the fo'c'sle. Heating by hot water is a very good system provided one can ensure good circulation and the radiators are fitted at the bottom of the compartment. This is difficult to arrange in single-tier ships' fo'c'sles. Usually the heaters have to be fitted overhead, and this is a bad place for heaters, whether of the hot-water or the electrical type.

I am very much impressed by the advantages of electric heaters. In dealing with this type of system

the author compares the temperatures obtained with a certain size of heating plant in a room, and the greater temperatures obtained with a larger heating installation designed to meet some new requirement. How much more power would he allow in the generating plant of, say, a 20 000-ton passenger and cargo ship in order to meet the new regulations?

The selection of the most suitable type of electric heater is affected by the particular position for which it is intended. In ships one has to try to place the heaters so as to suit all the other fittings which have to be placed in the rooms, and it is sometimes almost impossible to get the heaters in position. For example, I saw a case recently where three heaters were grouped around the head of a bed. In a small room one heater will probably be sufficient, but the larger the room the larger the number of heaters. On board ship the total amount of heat supplied may be ample, yet one may get cold corners and therefore complaints. Increasing the number of heaters means increasing the cost, but that is really the problem of the builder.

The author mentions the question of ventilation. It is obvious that it is not much use to switch on heaters when the ventilators, windows, and doors are open. The size of the heating installation must be based on the assumption that the portholes and doors are closed and the ventilators trimmed away from the wind. There are four common types of ventilators: cowl, mushroom, torpedo, and Fyffe, the last three being exhaust ventilators. If the wind is blowing in a certain direction a strong air current will pass down these ventilators. Heat loss under these conditions is not taken into account when fixing the size of the heating installation.

Mr. M. C. Cooper: It seems to me to be rather important, on account of the question of condensation, to maintain as far as possible a constant room temperature, and I should like to know how electric heating compares with steam heating as regards maintaining constant-temperature conditions.

There must be a certain amount of heat radiated from the engines while the ship is actually running. I should like to know whether this helps appreciably to warm the ship, and therefore whether it is taken into account in planning the general heating system.

Mr. A. H. Wilson (*communicated*): Changes from tropical to arctic conditions occur on board ship in much less time than 1 week. For example, on a voyage north from the West Indies to New York or Boston a drop of 30 deg. F. in a period of the order of minutes is experienced in winter time, when running out of the Gulf Stream into the Labrador current. When crossing from Glasgow to Montreal or Quebec in summer, it can be very cold in the neighbourhood of the ice and yet 2 days later, in the St. Lawrence River, the temperature is frequently above 90° F. These are two much-traversed routes. Weather changes on a voyage from New York to Bermuda can be severe, and a round voyage is made in 4 days.

With regard to natural cooling due to the vessel's speed, conditions of about 20 knots into a 40- or 50-m.p.h. gale or 20 m.p.h. in the same direction as that of travel, making dead air, are conditions likely to be encountered.

Because radiant heat causes "hot spots" I consider convection heating to be more suitable. This method gets down to the deck and is not "shielded" to the same extent as radiation would be in, say, crew spaces where there are tables, forms, and bunks. So long as the air is warm, furniture, etc., can be allowed a little more time in which to heat up.

A point that is apparently overlooked when overhead radiant heaters are suggested for crew spaces is that it is usually the hands and feet that need warming first, and these would be farthest from the source of heat. Caps (uniform) are often worn in accommodation, so that if need be the head could look after itself.

The convection heater which the author describes appears to be a desirable appliance, and I prefer this type of heater for use at sea. It is admitted that the right temperature is not the only essential to the maintenance of good health over extended periods of service abroad. We must also have correct humidity, and fresh air. Artificial heating without these two essentials causes throat and chest trouble.

As regards the correct temperature for crew spaces, I would draw attention to the case of a fireman coming off watch from the stokehold where the temperature may well be over 90° F. in very cold weather. Such a man would feel cold at a temperature of 60° F. Presumably the author's remarks in this connection refer to motor ships.

Most vessels have natural ventilation for crew spaces, and it is reasonably fair to ask for heating to be such as will maintain comfort with the Board of Trade minimum area of ventilation per man, namely 3 sq. in. inlet and 3 sq. in. outlet. Why, in estimating, are the passengers allowed 1 200 cu. ft. of air per hour per person, and the crew only 830 cu. ft.? Many of the engine-room staff surely need the higher figure to maintain good health, in view of the fact that atmospheric conditions in working spaces are not always good, e.g. in coal bunkers or motor-ship engine-rooms when the fans are stopped so as to maintain a reasonable temperature.

Heat loss by radiation in passenger spaces is not serious when compared with the corresponding figure for crew spaces on tramp and cargo vessels, on account of the nature of the material forming the bulkheads or lining. As the author suggests, heat loss due to radiation can be reduced by lining the steel surfaces with a material which is a non-conductor of heat—sprayed asbestos might be suitable if the exposed surface were made non-hydroscopic and vermin-proof—and a serious loss is thus reduced. Cork-dusting in crew spaces is prohibited by the Board of Trade.

Another problem which presents itself with cold surfaces, even though the space be warm, is condensation. I have experienced this in a room over a refrigerated chamber; the room was otherwise comfortable.

Useful information as to the heating conditions on merchant ships might be obtained from a questionnaire issued to all ratings on some representative vessels employed on various trades and routes. Men standing by a vessel in course of construction can sometimes give constructive criticism, which is frequently decried when offered.

The Board of Trade standard of heating is probably arrived at after consultation with all the interests concerned, and is designed to ensure that a vessel can go

anywhere, which in fact a large majority of cargo vessels do. The author apparently suggests designing the heating arrangements to suit a particular voyage. Would this be acceptable to owners generally, who may be called upon at some later date to modify the heating arrangements to suit, say, one voyage to a different part of the world?

It is not accepted that cargo is the equivalent of a body heated sufficiently to maintain a satisfactory degree of comfort for several days on going into, say, the English climate from the tropics; the effect is there to some degree, of course, but not many spaces are in contact with the holds. Spaces such as the engine room and the galleys must be heat-insulated in most cases. Also, after a period in the tropics most people feel chilly when the temperature falls to 70° F.

Important difficulties regarding the carrying-out of heating tests after one or two voyages are (i) that the crew might refuse to sail in the vessel because of inefficient heating arrangements, and (ii) the question of responsibility for the equipment in the meantime.

I do not think the electric heater that will not give rise to risk of fire has yet been designed. Passengers and crew insist on using heaters as clothes-driers; they are used in such a way as to restrict radiation, and overheating occurs as a result. The Board of Trade have done something to ease this situation by requiring a properly heated drying-room for the use of the crew.

Some shipbuilders do not consider seriously the fitting of electric heating, on the grounds of high cost. They are not directly concerned with the vessel after it has passed the Board of Trade tests, and hence they usually consider capital costs only; running and maintenance costs are not of very great concern to them.

I think many superintendents will contest the statement that the cost of electric heating is less than that of other systems; perhaps the statement needs qualifying.

Thermostatic control is a good method of heat-regulation, but it is to be expected that it will increase the cost of the heating installation, and I doubt whether owners, particularly cargo-ship owners, would be prepared to face the extra capital cost.

[The author's reply to this discussion will be found on page 444.]

NORTH-EASTERN CENTRE, AT NEWCASTLE, 28TH NOVEMBER, 1938

Mr. J. Eden: Evidence abounds now to show that electric heating is both practical and economical, but any suggestion to apply this system is still subject to considerable adverse criticism; and it is important, therefore, to ensure that where it is adopted the right scheme is installed. I am not satisfied that the direct electric panel system is the final answer to the general problem of electric heating in ships. A low-temperature panel system is usually the best system for the office type of building with its high thermal capacity, and in such cases continuous heating will be the most effective and economical method. For the factory type of building the unit air-heater system generally is favoured, because by this means the heat can be directed to a particular location and can be produced quickly; there is little advantage in warming the structure, which usually has a small thermal capacity. Outside temperature variations in both cases will require proportionate variations in the heat input to maintain a specific condition of comfort, but driving wind and rain, which is the worst condition, will adversely affect the factory most. A ship's structure is more comparable with a factory structure than with an office building, and has the disadvantage that the heat loss is always above the average for a similar stationary unit because of the continuous movement through the air and the sea. Because of this it would seem that any heating arrangement such as a radiant heating system that will transfer a high proportion of its heat output by direct or indirect radiation to the structure will be at a definite disadvantage. Some form of local electric unit air-heater will be more economical and equally effective. By the provision of localized manually-operated controls the individual requirements of the personnel could be accommodated best. The fan unit also would be of service in the tropics and during the warmer periods in other altitudes.

Flexibility of any ship's warming arrangement would appear to be of primary importance because of the varying climatic conditions to which any ship may be subject, and also because of the possible wide variation in the requirements of a ship's company, especially in the case of a passenger liner.

Mr. F. A. Ross: The author refers to a ship trading in the Baltic, where, according to his calculations in Table 5, in order to make up the heat loss from steel bulkheads, etc., a 4·5-kW heater is required in a space of 450 cu. ft. These figures are so alarming that they require careful examination. The author gives three sets of figures in Table 5, for outside temperatures of 60° F., 25° F., and 15° F.; and if we take 25° F. as the average outside temperature we find that a heater of 3-kW capacity is required. With electricity at 0·5d. per unit for the three winter months this would mean a cost of £13 10s. to heat this small room; and during a ship's life of 25 years the cost would be £337. It is obvious from these figures that, for ships trading in cold climates, it is essential in the first place to line all crew's quarters with wood panels, and for very cold conditions it may be necessary to heat-insulate the bulkheads with cork, aluminium foil, glass wool, or any other good heat-insulating material. If the bo'sun's room referred to in the paper had been heat-insulated with cork slabs, according to the figures given in Table 1 the heat loss would have been reduced to one-sixth or one-seventh of the figure for painted steel. The initial cost of heat-insulating this cabin would have been recovered within 6 months, whilst heat comfort and healthy conditions would have been provided for the occupant and considerable saving in cost made by the owners.

I do not agree with the author's statement that steam heating is very nearly ideal for the heating of a ship: first, because it is costly to install; secondly, since the heat losses are excessive owing to the long lengths of

steam piping which radiate heat, and dissipate heat by conduction, into the ship's structure; thirdly, because the steam heater in itself is wasteful and the upkeep of piping, joints, etc., excessive; and finally, because the radiant-heat wavelength, from a heater at steam temperature, situated in a small cabin, is the worst type of radiant heat to which the human body can be subjected. This particular wavelength sets up congestion of the blood vessels on the exposed skin of the face, and causes headache. It also affects the nose and throat membranes, producing a feeling of choking. If the author's statement that steam heating is nearly ideal is based upon the assumption that there has been in the past an abundant supply of steam, and that, because of this, the heat waste went on unchecked and unnoticed, then clearly this condition does not now prevail on Diesel-driven vessels. On the other hand, he may have arrived at his conclusions from his curves of heat emission shown in Fig. 2. To my mind the curve for the steam radiator may easily be misleading, as it shows the heat emission with an ambient-temperature variation from 10° F. to 70° F. In the problem confronting us we are primarily concerned in maintaining heat-comfort temperature (55°–65° F.) in a room, or, if the installation is properly designed with thermostatic control, in ensuring that the temperature variation shall not exceed 2 deg. to 3 deg. F. According to the curve, the inherent regulation of the steam radiator under normal conditions of service (and even allowing a temperature variation in the spaces of 10 deg. F.) amounts only to 150 B.Th.U. per hour, and at 2 deg. F. variation to 30 B.Th.U. per hour, which is of little practical value. Further, if this steam heater had been fitted in the bo'sun's room previously referred to, this additional heat emission would have been given off as radiant heat and absorbed by the ship's structure; so that, instead of the apparent extra heat being useful heat, it results in a further loss and expenditure of steam. Normally, 70 % of the heat given off by a steam heater is convected heat and 30 % radiant heat. In a non-insulated cabin the radiant heat is absorbed by the steel bulkheads and is thus completely lost. Further, the air speed of the convected heat is not sufficient to cause proper air circulation, and consequently the heat from the slowly rising warm air is also absorbed by contact with the cold steel bulkhead, so that the number of B.Th.U. required by the steam radiator to provide a comfortable temperature is considerably greater than that required by a properly designed electric convector. The author next deals with the question of radiant heat versus convected heat, and states that in spaces with steel walls, such as crews' wash-places, radiant heat is essential. I agree with the author that a certain degree of heat-comfort can be provided by radiant heat, for intermittent use in wash-places, but it is essential that the radiant-heat rays be projected from high-temperature reflector heaters, with the beam directed to flood-heat the body, otherwise it is of very little value. If normally it is allowed to fall on the steel deck or bulkheads the whole of the radiant heat is absorbed and conducted away from the compartment very quickly. Assuming radiant heat is to be used in spaces which are to be occupied intermittently, and which normally are to be kept at a reasonably high temperature, then the best arrangement is to use dual heaters, whereby the heat can be projected as radiant heat when the space is occupied and, by closing louvres in front of the projectors, the radiant heat is absorbed and transformed into convected heat, causing a circulating flow of warm air. I have successfully fitted a number of these dual heaters, not only on board ship but also in drawing offices, where it is necessary to produce maximum heat comfort to obtain the best results from highly paid men. For general use throughout a ship there appears to be only one practical solution to the problem of space heating, and that is by the circulation of warm air. In the past a system of mechanical ventilation and heating has been used, wherein the air at the fan inlet was blown over steam-heated tubes, and through a metal trunking system into the cabins and public rooms. The method now being adopted is to fit properly-designed electric convectors in the public rooms and cabins, so that the passenger has direct control of the heat required. I have compared these two systems for cargo and passenger vessels from the point of view of installation and consumption costs and in each case have found the electric-convector system, complete with all cables and other apparatus, to be cheaper in first cost than steam-heated coils and their component steam pipes, etc. (i.e. excluding the cost of the fans and trunking for the ventilating system which may be required for ventilation and cooling purposes in warm climates). From the point of view of running cost, the number of B.Th.U. required in the steam-heated apparatus to provide an equivalent air temperature in the cabins can be more than 10 times that required with suitable electric convectors fitted in the cabins.

This great difference is due to the fact that a large quantity of cold air has to be raised to approximately steam temperature and then driven at a high velocity through the long lengths of air trunks. The heat losses in the steam pipes supplying the heating coils at the fans (which are very often fitted on top decks) are considerable, and the heat losses from the air passing through the trunking system at an appreciable velocity are also very great; whereas electric convectors located in cabins and spaces where the heat is actually required involve no transmission losses, and, as air-heaters, they can be made practically 100 % efficient. I originally designed special convectors for ship use, wherein the heating element is located in a centre tube having a converging nozzle formation. This centre tube is surrounded or lagged by an auxiliary air duct or ducts to reduce surface temperature and radiation and ensure that the total heat generated shall be given up to the air. The nozzle arrangement automatically imparts a velocity of 180 to 200 ft. per minute to the issuing air, which mixes with the cold air in a room and rapidly raises the air temperature. This air velocity in a cabin or space with a relatively low ceiling is very important, as it is found that with an issuing air flow of 180 ft. per min. the warm air is circulated round the cabin and drawn back through the heater, being re-warmed and re-circulated, thus ensuring that the heat loss is a minimum. With this arrangement a loading of 1 to 1·5 watts per cu. ft. is required to provide and maintain heat comfort in a cabin with normal ventilation. The

air speed of 180 ft. per min. is most important in maintaining air freshness and heat comfort.

I agree with the author that the B.O.T. regulation which stipulates a temperature-rise from 30° F. to 60° F. without defining how this is to be measured, is unsatisfactory. The thermometer readings in a small cabin can be entirely misleading, depending largely upon the type of heater used, and may result in the introduction of wasteful methods of heating. For instance, in the wash-places heated by radiant heat which were referred to previously, when the thermometer is placed in the beam of radiant heat the reading can easily reach 80° F. at a distance of 4 to 5 ft., and anyone standing in this beam would be perfectly comfortable; but if the thermometer be taken out of the beam the temperature reading will be the air temperature of the space, which will be approximately the temperature of the steel bulkhead, and can easily be 30 to 40 deg. F. less than the beam temperature. Again, in a room with painted steel bulkheads which is heated by warm air, if the thermometer is suspended close to the bulkhead the reading obtained will be approximately the bulkhead temperature and it will be quite impossible to get a temperature reading in this position equal to the air temperature in the centre of the room. Further, if a trunking-and-fan ventilating system has been installed, to supply 10 to 15 air-changes per hour for cooling the cabins, and to provide air-freshness and the cooling effect of moving air in the tropics, then if this ventilating system is allowed to function during a heat test in a winter climate without any stipulation as to the quantity of cold air to be admitted, the heat required to raise the temperature of a mass of cold air to 60° F. (and which is then to be blown out of the ship or extracted by the exhaust fans in lavatories) can constitute a very serious loss to the shipowner and necessitate a heating plant out of all proportion to the requirements. The first essential, therefore, is to determine the quantity of air required in a cabin during occupancy. It is known that the air in a sealed room of 1 000 cu. ft. capacity will keep 5 men for 12 hours; therefore the actual amount of fresh air required per man to maintain healthy conditions in a room of this size is something considerably less than 1 air-change per hour, and in service during cold weather this is normally what the occupant of the cabin would permit. The B.O.T. regulations do not specify the amount of air to be supplied, but state that an area of 3 sq. in. of inlet and outlet is to be allowed per man in the natural ventilation trunks, which usually supply air into the passages and exhaust from the rooms. This opening has to give an adequate supply of fresh air in a warm climate, and in the past has satisfied requirements. The amount of air which would enter a cabin with the natural ventilation apertures stated above, would produce, under most conditions, very much less than 1 air-change per hour, so that this confirms the actual air requirements as being much less than the figure of 830 cu. ft. per man stated in the paper. Taking all these points into consideration, it appears that considerable saving could be effected in the heating of ships if the recommendations of the Institution of Heating and Ventilating Engineers were used as the standard heat test for ships. It would then become necessary to heat-insulate the cabins, but the cost incurred would be outbalanced by the reduction in expenditure for plant and the considerable saving in the cost of heating.

Mr. W. Richardson: I notice that under the extreme conditions sometimes met with at sea the author requires a loading of 4·5 kW for an uninsulated room of 450 cu. ft., which is 6 times the figure required for a normal building heating installation. It is obvious that under these conditions satisfactory heating is almost an impossibility, owing to the large temperature variations which must occur in a comparatively small space. A certain amount of heat insulation is therefore a necessity if comfortable conditions are to be maintained in rooms of this nature, and I suggest that the author should add to his paper figures showing the cost of heating rooms of this kind, both with and without insulation, so as to convince shipowners that heat insulation is a sound commercial proposition. Later in the paper he refers to the heat absorption of a ship's structure in the tropics, and the discomfort caused by the radiation from exposed steel surfaces at temperatures which may reach 150° F. A little heat insulation would reduce this radiation and greatly improve the comfort conditions in the room.

The author states that the use of air conditioning will become universal on high-class passenger ships; it must be remembered that this plant would have a dual function—heating the air in cold weather, and cooling and reducing its humidity in the tropics. This double function can be carried out by means of electricity.

In 1929 Mr. Haldane read a paper* on the use of the heat pump, or reversed refrigeration cycle, and pointed out that efficiencies of 250 % to 400 % could be obtained by this means under favourable conditions. I should like to ask the author whether he has considered the use of the heat pump as an alternative to steam heating on board ship. There would be no technical difficulty in applying the heat-pump system, seeing that the sea water would furnish an unlimited supply of low-grade heat. Several buildings in the United States are heated in the winter and cooled in the summer by a plant of this type.

For local heating of individual cabins on a passenger vessel the author favours radiant-type as opposed to convector-type heaters, and points out the difficulty of utilizing high-temperature heating sources on account of the fire risk, and the difficulty of mounting. The low-temperature radiant-heat ceiling panel, utilizing hot water, has been very successful in building heating. As it is fixed on the ceiling, ample space is available for fixing, and the low temperatures necessary would avoid danger of fire. It should be possible for the electrical industry to evolve a low-temperature electrically-heated ceiling panel which is fireproof, easily installed, and reasonably cheap.

Mr. J. Crawford: My experience in the manufacture of electric heating apparatus for ships and buildings suggests that it is the method of applying the heat which is of importance. It is quite possible to generate heat in a ship and then lose the greater part of it through the use of a wrong method of application. It is very important that the heat should be generated where it is to be applied, namely in the cabins, public rooms, etc.;

* *Journal I.E.E.*, 1930, vol. 68, p. 666.

and, further, it is necessary that the heat should be in the form of circulating warm air, which if possible should be reheated in the space and not blown out by the use of pressure fans, or allowed to escape. To raise the air temperature of a cabin or room effectually a convector is necessary which will give an air velocity of roughly 200 ft. per min., with an air temperature of 150°–180° F., and in order to save first costs this velocity must be obtained without the use of a circulating fan. Such a convector has given excellent results on ships plying in all sorts of temperature conditions, e.g. on Canadian lake steamers where the conditions were as bad as, if not worse than, the conditions mentioned by the author as having been met with in the Baltic; in this case the loading allowance was 1·5 watts per cu. ft. of space, and the cabins were heat-insulated. I would state that high heating efficiency and rapid air circulation cannot be obtained with the ordinary box-type convector fitted with a low-temperature heating element. The convector to which I have referred embodies a special design of mesh element in conjunction with a specially-designed air nozzle.

Radiant heat may be employed to a limited extent as a useful addition to convected air in such places as public rooms and for the benefit of the officers and crew. It is necessary to have some form of radiant heat to remove the chill from the hands and feet, which suffer most in cold weather.

The author refers to the practice of fitting an electric heater at the end of the air trunks supplying cabins, and mentions the danger that the air may be shut off, with the result that the temperature of the heating element rises to a dangerous value. Provided this danger can be eliminated the method is an ideal one, where air trunking is available or already installed.

An air heater which eliminates the danger of rise in element temperature has been produced, and tried out in a temporary ship's cabin. Its advantages are: (*a*) Individual control. (*b*) A varying supply and volume of cool air (including re-conditioned air, if available). (*c*) A varying supply of warm air. (*d*) The air supply can be entirely shut off, if conditions warrant; and the air in the cabin can be circulated, reheated, and re-circulated, as in the convector system.

[The author's reply to this discussion will be found on page 444.]

MERSEY AND NORTH WALES (LIVERPOOL) CENTRE, AT LIVERPOOL, 12TH DECEMBER, 1938

Mr. A. G. Barnard: The paper may be divided into two main sections: first, the basis and methods of estimating the heating required; and secondly, the type of electrical heater most suitable for the duty.

An estimate of the heating required is just as necessary where electric heating is being installed as it is in the application of steam-radiator or hot-air heating systems, but as it is not quite so simple a matter in the case of electric heating to install plant with generous margins it is all the more important that we should be able to make an accurate estimate of the actual requirements. It is apparent from the author's analysis of this matter that there is no simple relation between the volume of the space and the watts necessary for heating, and there appears to be no procedure available other than the calculation of the leakage losses through the bulkheads, decks, etc., of each individual case.

In this connection, I can see no reason why the heat-loss coefficients should be shown in Table 1 as being affected by the severity of the weather. Severe weather, for example, would involve a larger temperature-difference, while strong winds might be as readily associated with mild weather. While it is usual with heating engineers to make allowance for "exposure" in the case of a ship, all exposed surfaces must be treated in the same fashion. Heating engineers regard the coefficients of heat transmission as being made up of three factors: (1) an outside skin coefficient which is affected by wind; (2) a coefficient governed by the resistance to heat flow through the surface; and (3) an outside skin coefficient. On the basis of the data available the coefficients given under the heading "Mild conditions" in Table 1 are sufficient for the most severe weather, the larger temperature-difference in the case of the severer weather being sufficient to allow for the additional heating load. It would be of interest, therefore, to learn the author's reasons for introducing the higher coefficients.

It is impossible to develop coefficients from tests made in actual practice, on account of the impossibility of maintaining constant conditions on which to base these coefficients, and because, frequently, insufficient information is available. For example, in the captain's report referred to on page 422 of the paper, no reference is made to the actual temperature existing in the bo'sun's cabin. That a sheet of ice formed at the foot of his bunk might suggest that the temperature was below freezing point, although we are all familiar with ice formation on windows in very warm rooms. I am of the opinion that an important factor contributing to the discomfort of the room was the cold surfaces, such as the ship's side, and the radiant effect from these. It is well known that, where panel systems of heating are used, the air in the room can be at a lower temperature than is possible with heating systems of other types, the reason being that the radiant effect of the hot panels compensates for the lower-temperature atmosphere. It is also likely, therefore, that where the walls are very cold the air of the room will be at very much higher temperatures than would be necessary with lined or insulated walls.

Another factor which I do not think has been sufficiently stressed is ventilation. Where natural ventilation exists or where there may be frets or jaloused doors, the air-changes in the space may be altogether outside control and would tend to increase with increase in the temperature-difference. In this connection I would draw attention to the conclusions drawn from Test 2 on page 433, where the allowance for ventilation only amounts to about 400 cu. ft. of air per hour per man, less than half the B.O.T. requirements.

I suggest that for electrical engineers, factors giving the wattage direct would be preferable to the coefficients used by heating engineers. The following coefficients, multiplied by the temperature-difference, give the wattage direct:—

Bare steel or glass, exposed	0·4
Bare steel (exposed), with air space, and lined	0·26
Bare steel (internal)	0·2
Exposed deck, wood over steel	0·13
Deck floor, $2\frac{1}{2}$ in. wood	0·1
Deck floor, 1 in. composition	0·13
Single wood partition	0·15
Double wood partition	0·1

$$\text{Ventilation factor} = \frac{V}{180} \times \text{Number of changes per hour,}$$

where V = volume of space.

The question of insulation is of extreme importance in connection with electric heating. If a bare-steel coefficient were reduced from 1·3 to 0·5 by insulation, the saving would be 7 watts per sq. ft.; thus, insulating 143 sq. ft. of steel surface would save 1 unit. In 2 400 hours (100 days of full heating), £10 would be saved, on the basis of 1d. per unit. This would amount to $\frac{1}{5}$d. per sq. ft., which would go a good way towards the cost of insulating the surfaces available.

It is apparent that the author prefers the radiant type of electric heater, and I am inclined to agree that it gives the best results for the minimum amount of expenditure. It has great advantages for use in cabins with steel surfaces, as it tends to neutralize the radiant effect of the cold walls. It is unfortunate that the regulations should show a tendency to discourage radiant-type heaters, as it would be of great advantage to encourage the development of a radiant type which would not have the disadvantage of contributing to fire hazards.

I consider that a low-temperature tubular type of electric radiator would give the best all-round results, as while a large proportion of its heating would be due to convection it would still have a radiant effect. Although these radiators tend to be bulky, there is no reason why space should not be found for them between the beams at the ceiling, the result being in effect a form of panel heating. This form of heating could only be regarded as suitable for crews' quarters, on account of its appearance.

Mr. J. Cormack: In introducing his paper the author remarked that one of the arguments in favour of the use of electricity for heating aboard ship was the ease with which electric cables may be carried round bends and the smallness of the space occupied by them. This argument, however, will be ruled out if, as is stated under the heading "Methods of Heating" (page 426), "it is only a matter of time until air-conditioning is fitted." Air-conditioning will necessitate the use of rather bulky and costly heat-insulated air trunking.

In the section dealing with "Ventilation Losses" (page 425) it is stated that "ventilation to staterooms is supply ventilation and it blows out into the alleyways and travels thence to various parts, principally the lavatories and baths. . . . This warmed air from the staterooms may be relied upon to make the lavatories and baths fairly comfortable." In the very next sentence, however, it is stated that "baths, and lavatories, all have exhaust ventilation and take a heavy toll of heat." The first statement suggests that there is no difficulty in heating lavatories and baths, whilst the second implies that the reverse is the case. Perhaps the author could elucidate the apparent inconsistency.

Finally, in the Appendix the "Actual Losses" are given in kilowatts and also in B.Th.U. Since the former is a power unit and the latter an energy unit, the two values cannot be equated. I assume that in the second case B.Th.U. per hour is intended.*

Mr. F. L. Bullen: I notice that Table 1 gives a smaller coefficient for a wood deck on steel under accommodation than for a similar deck over accommodation. I should have thought the two values would have been the same, but perhaps the difference is due to the exposure factor.

With reference to waste-heat boilers, so far as my experience goes, waste heat is always available when a ship is at sea, and even when the machinery has been slowed down there is sufficient to give all the heat required. When a ship is in port, there are no exhaust gases, and the waste-heat boilers are then heated by oil fires.

I do not support the statement that steam heating is difficult to arrange in ships with superheated steam, because I know of a ship using superheated steam for the main engines on which a very effective heating system is installed.

Radiant heating seems to be ideal, if it can be obtained, but in practice only those sitting in front of an electric radiator get the benefit of the heat. Some method of covering a greater area is required: perhaps panel heating would be more satisfactory.

Warming by conduction might be a useful method; possibly some means might be developed for warming the floors of a ship. This is the method adopted for heating Liverpool Cathedral, and it seems to be very satisfactory.

It is stated that the cost of installing panel heating would be prohibitive; I should like to know why panel heaters are so expensive.

It is also mentioned that heating by warm air is wasteful, and the author suggests that the waste could be reduced by arranging the ventilation system to give only 3 changes of air per hour. If 12 changes per hour are required in normal weather, about the same will also be required in cold weather, because the body continues to give off moisture and carbon dioxide. To cut down the ventilation to 3 changes per hour would tend to make the conditions somewhat uncomfortable, and it would appear that it is necessary to install sufficient heating to allow for the normal number of changes of air under all conditions.

Mr. J. McGavin: The final criterion of comfort on board ship is the effect of the air temperature and the temperature of the surrounding walls and furniture on the human body. Perhaps the author would give some indication as to the sensitivity of the Eupatheometer compared with that of the human body. Some such

* Corrected for the *Journal*.

figure seems to be required in order to correlate the test figures with the conditions of comfort likely to be experienced in any particular case.

Mr. C. M. Kelly: The author mentioned that if panelling were adopted for lining the ship's sides its cost would very soon be saved in reduction of heat losses, but that objections to panelling are sometimes raised on account of its proclivities for harbouring and encouraging vermin. I should like to know what material is usually used for putting behind the panel, and whether some of the new verminproof materials such as glass silk have been tried. These would tend to drive vermin away rather than encourage them to make a home there.

Mr. G. L. Williams: The author states that the capital cost of electrical heating installations for ships compares very favourably with the cost of other available methods. I have noticed that in the case of buildings the capital cost averages about £5 per kW of heating installed, and I should be interested to learn what the figure is for merchant ships, and also the relative costs of other systems of heating now in use.

The use of thermostats is advocated for temperature control. It would appear that the positioning of the thermostats calls for some care in the case of ships if satisfactory results are to be obtained, for owing to the wide differences in the thermal transmittances of the materials forming the boundaries of the rooms to be heated there will be a tendency for steep temperature-gradients to be set up. The effect of this on an incorrectly placed thermostat would be that the thermostat would gain a wrong impression of the condition of warmth existing in the room under its control, and overheating would result.

An objection to the use of radiant panels, which is particularly applicable in ships, where headroom is generally limited, is that people frequently complain of sick headaches and lassitude when they are subjected to the radiation of these panels for any appreciable period.

As regulations and other considerations rule out the use of radiant heaters in general—chiefly, it is noted, on account of the presence of a high-temperature element—the convector with its element operating at black heat is apparently the most suitable type of electric heater for use in ships.

[The author's reply to this discussion will be found below on this page.]

TEES-SIDE SUB-CENTRE, AT MIDDLESBROUGH, 4TH JANUARY, 1939

Mr. H. W. Townshend: The importance of applying some form of insulation to steel surfaces in living spaces exposed to the weather cannot be over-emphasized. Some shipowners have adopted the scheme of spraying asbestos on to the under-side of decks and vertical surfaces where these are exposed to the weather; the value of this scheme from the point of view of the retention of heat in cold climates and the insulating value in hot climates has proved highly satisfactory.

It has been a requirement of the Norwegian authorities (Sjøfartskontoret) for many years now that steel surfaces of crews' accommodation exposed to the weather " shall be covered inside with wooden panelling but in such a manner that the wooden panelling shall not rest immediately on the iron. Under the panelling against the ship's side shall be placed felt, millboard, or the like."

On British fishing vessels it is an established practice to line the ship's side in crews' accommodation with wood, and this has been found particularly necessary on vessels engaged in fishing in Arctic regions.

In spite of the Board of Trades insistence on ventilation in crews' accommodation, in vessels working in the Arctic and Antarctic it is quite common to find that the crew have sewn canvas covers over all the ventilators. Members of the crew state that they get all the fresh air they require when on duty, and do not want more when they come inside. It has also been found under these circumstances that coal-fired stoves are more satisfactory for heating than steam radiators, which cause undue sweating and result in bedclothes becoming very damp.

I agree with the author that waste heat from the engines is not a reliable medium for the heating of ships; but it is quite common to employ oil firing to augment the waste heat, and in such cases the same boiler is also used for driving some of the engine-room auxiliaries and the steering gear.

Instances can be given of recently-built Diesel-electric-driven and Diesel-driven vessels where hot-water heating was installed and the generators consisted of oil-fired hot-water boilers, thermostatically regulated.

With regard to the " soaking time " referred to in the paper, shipbuilders generally are aware of this feature and usually delay carrying out the Board of Trade tests as long as possible—in any case until the heating has been on for some considerable time. Does the author consider that dampness in any way affects this " soaking time," so that heating conditions are improved as the various compartments are thoroughly dried out?

THE AUTHOR'S REPLY TO THE DISCUSSIONS

Mr. H. C. MacEwan (*in reply*): The strongest criticism of the paper is contained in Mr. Wilson's contribution, where his suggestion of issuing a questionnaire to ratings on representative vessels indicates that he regards the paper as theoretical rather than practical. In actual fact, the paper is based not only upon my own experience and tests at sea, but also upon that of sea-going engineers and electricians who have given me, over a period of years, valuable information of extreme conditions they have met. As Mr. Wilson indicates, sea-going men are very keen observers and, even when they are unable to interpret scientifically what they have observed, their facts are usually accurate. He may rest assured that their observations have been given proper appreciation.

Mr. Kempster points out that some ships experience the ordinary seasons, and that applies to coastal ships

and others whose route does not vary much in longitude and is not very far afield. Mr. Wilson's observations in this respect are interesting.

Design for a Particular Trade.

Mr. Watson and others consider it unsafe to design the heating installation purely to suit the trade route for which the vessel is built, but the same objection might be raised in regard to many other things on board ship, and shipowners tell us that the cost of ships is already too heavy. Unnecessary expense must therefore be avoided and it is clearly uneconomic to install heating which will probably never be required. Further, as the heating installation is designed for the worst conditions likely to be experienced and the calculations, like any other calculations in engineering design, include some factor, or surplus, of safety, unless the amounts are cut unduly fine there will be sufficient capacity in hand to meet an occasional heavy demand without severe effects. Moreover, when heating is required on board ship the heat is kept on continuously day and night and has therefore only to replace the heat losses: it does not have to bring the temperature up each morning from a cold to a comfort state. Heating to excess once every 24 hours is not, therefore, a normal requirement as in most buildings, and the excess amounts in hand are available for occasional heavy requirements.

Heat Reserves.

Some apprehension has been shown in regard to the acceptance of heat reserve as a factor in calculation. While it is true that galleys and machinery spaces are heat-insulated, none the less the effects of their warmth are felt; in fact the warmth from them, through the structure of the ship, can sometimes be traced on the bare steel, even by the hand. It is not suggested that "cargo is the equivalent of a body heated sufficiently to maintain a satisfactory degree of comfort for several days on going into, say, the English climate from the tropics," but that the effects are both real and appreciable is clearly demonstrated by the fact that hot-weather ships need very much less in watts per cubic foot than is necessary on ships which do not have such heat reserves.

The above replies to Mr. Cooper's question, but the heat from the engine room is not usually included as a factor in calculations, unless in respect of a space where the bulkhead divides it off from the engine room.

Heat-loss Coefficients.

In regard to the heat-loss coefficients, Mr. Barnard suggests that watts loss would be better than B.Th.U. loss. Personally I prefer to use the latter, as it is the usual basic unit in heating calculations. Mr. Barnard also questions the reason for using the three different values in Table 1. As he states, it is impossible to develop coefficients from tests made in actual practice; but it quickly became apparent, in investigating the subject, that a range of coefficients was necessary if successful calculation was to be made for the widely varying conditions met in different parts of the world and on different trades. Table 1 is an attempt to provide this, and it is designed to give quantities which will deal with the most severe conditions experienced by ships sailing into the three categories of weather shown at the head of the three columns. The coefficients cover the maximum effect of the strong winds, heavy seas, low temperature, and rain or sleet, usually met with under those categories. In answer to Mr. Kempster, they are designed to be applicable to all ordinary ships; and in reply to Mr. Bittles, the difference in plate thickness found on board merchant ships would not appreciably affect the calculations.

In regard to Mr. Bullen's question, the small difference in coefficient between wood with steel below and steel with wood below is due to the effects of conduction.

Crews' Quarters.

Mr. Savage considers that the heating in the crews' spaces should be equal to that in the passenger accommodation. I am not in agreement there, as the passenger accommodation is over-supplied with heat but has stewards to control the heaters as necessary to obviate overheating. I do not advocate working down to the Board of Trade minimum in crews' quarters; but, as stated in the paper, it is, in my opinion (and this, it will be noted, is supported by Mr. Watson), no kindness to overheat crews' quarters. It must be remembered that, as the calculations are made for the worst conditions which will be experienced, the spaces will normally be heated to a much higher temperature than 60° F.; for this reason Mr. Savage's fear that 80 % of the crew would be doing manual labour in an atmosphere of 65° to 70° F., and spending their leisure in an atmosphere of 60° F., is unlikely to be realized.

Ventilation Losses.

This difficult question has given rise to more discussion than any other particular point, but the general trend of opinion appears to support my conclusions. It is advisable to assume that the air is delivered into the vessel at outside temperature, except where heating is provided at the ventilation fans; and there is, as Mr. Kempster suggests, the equivalent of a load factor in the problem, which reduces the average figure to 3 air-changes per hour. The figures given are those which are found to be reasonable in practice.

Mr. Wilson criticizes the basis of 830 cu. ft. of air per hour for the crews' spaces and considers that 1 200 cu. ft. should be allowed as in passenger spaces. The difference in the amount of heating is small, but, as stated in the paper and confirmed by Mr. Townsend, it is doubtful whether any crew's room gets 830 cu. ft. of ventilating air per hour in cold weather. In reply to Mr. Barnard, it will be noted that instead of the calculated 3·65 kW the amount of heating installed was 5 kW. Actually, it is reported that a visit to the room showed the ventilators to be blocked up and that one heater of 2 kW is never used, as the room gets too hot with 5 kW on. Mr. Cormack queries an apparent inconsistency; my intention was to convey that while the warm air drawn from the alleyways by exhaust fans into the lavatories and baths keeps them fairly comfortable, it is exhausted from them into the atmosphere, carrying with it heat that is thereby thrown to loss. The ventilation quantities into these places are of course high. Mr. Cormack points out that the calculations show B.Th.U. instead

of B.Th.U per hour; this has been corrected for the *Journal*.

Thermal Comfort and the Insulating of Ships' Sides.

It is in the crew's quarters more than anywhere else on the ship that the basic principles of thermal comfort and the advantages of lining the ship's sides are most noticeable. Dr. Margaret Fishenden has established that the conditions of greatest thermal comfort are with an air temperature of 55° F. and requisite radiation, and that when the walls are at a temperature lower than 65° F. the air temperature must be raised to counteract the effects felt from the excess heat loss from the body to these cold walls. Mr. Bittles asks for some evidence to support the statement that air temperature is not a true measurement of proper and adequate heating; he will find what he requires in "The Equivalent Temperature of a Room and its Measurement" (Building Research Technical Paper No. 13), and the "The Heating of Rooms," by Dr. Margaret Fishenden (Fuel Research Board Technical Paper No. 12), both obtainable from H.M. Stationery Office.

In a room with a bulkhead or ship's side surface at a low temperature—say 40° F. or lower—it is almost impossible to secure conditions of comfort. The air may be at 65° or 70° F. but a feeling of chilliness will persist; ample ventilation at high temperature may be supplied, yet the room will be stuffy because the humidity is too low. The economy of lining the ship's side is therefore twofold, for not only are the heat losses substantially reduced thereby, but, in addition, comfort is secured with a lower air temperature.

In reply to Mr. McGavin's question, the Eupatheometer is approximately correct in its sensitivity and readings in conditions which give human comfort. Above and below that range its readings are not strictly correct under the new definition of equivalent temperature, but show that in sum total the surroundings are too warm or too cold.

In regard to Mr. Richardson's suggestion of adding figures of cost and savings secured, I prefer in dealing with marine questions on a general basis to make calculations in terms of quantities rather than money, since conditions vary so greatly on board ship, both in the capital and the running costs. Mr. Ross and Mr. Barnard have, however, supplied some interesting figures.

In reply to Mr. Kelly, usually there is only air space behind panelling on board ship, and it is this air space that harbours vermin. Something homogeneous and jointless is necessary as a preventative of insect breeding.

In reply to Mr. Cooper's question, as heat from electric heaters is almost instantly available in full measure the maintenance of constant temperature is much easier, and more even, with thermostatic control than with steam heating.

Heaters and Methods of Heating.

I am in agreement with those contributors to the discussions who have advocated supplying warmed ventilating air (particularly air that has been conditioned, since that eliminates the defects of low humidity and also secures the economies of re-circulation) and fitting a suitable makeweight of electric heating for adjustment of the temperature to suit local conditions and individual taste. Electric heating at the individual air outlets may be attractive when successful safeguards against overheating have been secured, but the important point is to avoid heating the air too much, and the use of individual heaters, particularly those with a good proportion of radiant heat, will always be necessary for the attainment of satisfactory warmth-comfort.

In reply to Mr. Bittles, Griffiths and Davis found by experiment that for practical purposes most pigments are alike with respect to heat radiation where heaters at normal surface temperature are concerned.

In saying on page 427 "that the action of a steam radiator is from the point of view of ship's heating very nearly ideal and very hard to equal with any other medium" I did not mean to convey, as Mr. Ross has inferred, that I regarded steam heating as ideal. I entirely agree with his objections to steam heating generally, and it will be seen that it was to the self-adjustment in a steam radiator that I referred, as distinct from the fixed amount of heat from an electric heater; that is to say, it is a comparison of the units and not of the systems. I note that he does not consider that the amount of self-regulation is worth while, but I am sure he will agree that every little helps when the total amount is too small, and under severe conditions the radiator with a fixed output would certainly be unable to cope with the situation as well as one which increased its output. It is also well known that when such a state of affairs occurs, the engineers raise the steam pressure to get still more out of the radiator. It will be appreciated that in drawing attention to these factors my object was to urge desirable improvements in electric heaters which I believe can be secured. I have no doubt whatever in my mind that a comparison of the two systems, as distinct from the units, shows electric heating to be very much the better all round, but that is no reason why it should not be better still.

Mr. Richardson asks whether the heat pump has been considered as an alternative to steam heating on board ship; as far as I know it has never been so applied. In the first place, it entails the use of hot-water heating, which is not as good as steam heating, and has great disadvantages compared with electric heating for use on board ship; further, fresh water and not salt water would certainly have to be used. In addition, most of the refrigeration on board large ships is CO_2 refrigeration, which does not give a good heat-pump effect.

In regard to the general question of the use of radiant heat, and in reply to Mr. Watson, I would say that I fully appreciate the readiness of the classification societies to consider new proposals, but my feeling is that where the rules of responsible and highly respected bodies like these definitely prohibit rather than safeguard the use of such equipment as radiant heaters valuable development is often throttled, because people with ideas naturally fear that to work upon them when such restrictions exist is a mere waste of time.

Thermostats.

As Mr. Williams observes, the positioning of thermostats requires care; nevertheless, satisfactory positions

can be found. In general, fixing the thermostat in the heater itself is not advisable, since it keeps the instrument constantly hunting and the number of operations is multiplied enormously. The attraction of this position for shipboard heaters lies in the elimination of fire risk, which is secured, as mentioned by Mr. Watson; but on the whole it would appear to be best to have one for this purpose in the heater, set fairly high, and another for controlling the air temperature, fixed in the space, both operating the same switch where direct current is used. The thermostat units for fire-risk elimination are obtainable now for less than 10s. each; the additional cost where space-heating thermostats are fitted is thus very small. One thermostat can be made to operate a number of heaters in alleyways or public rooms, but each cabin must, of course, have its own when it has its own heater.

Tests.

In reply to Mr. Townshend, dampness does undoubtedly increase the "soaking time" very appreciably. Unfortunately, on large ships it is not often that, in building, a stage is reached early enough to allow the doors to be shut and kept shut sufficiently long to give sufficient soaking time to heat up and dry out properly before the B.O.T. tests are made. In fact, I doubt very strongly whether it would be possible to reach a stable condition of heat content and normal temperatures otherwise than in the process of normal trade. Moreover, the question of cost is a very serious one. Take the case of a large passenger ship with heating equivalent to, say, 500–1 000 kW at 220 volts, i.e. say, 2 250–4 550 amperes. Is it reasonable to incur the cost of drying out the ship for a test by running the ship's generators night and day for a week, or of running heavy shore connections carrying electrical energy which in sum total will probably cost about 1d. per unit, when the results will be achieved without cost just as quickly in service?

Costs.

Where costs are questioned every ship has to be considered individually. Clearly a ship with steam generators consuming 20 lb. of steam per kWh gives very high running costs compared with Diesel generators producing electrical energy at $\frac{1}{4}$d. to $\frac{1}{2}$d. per unit. Again, the capital-cost questions depend upon lengths and facilities in relation to main cables and steam pipes and the layout of the ship. In designing a new ship the calculations are not difficult to make and the question can be settled fairly easily in the early stages. If additional generators were required for heating alone, electric heating would not be competitive, and, in reply to Mr. Shepherd, a 20 000-ton vessel for passengers and cargo would not require any additional generating capacity unless all the auxiliaries were steam-driven. If they were, then the additional capacity required would probably be the full amount of the heating installation, say 250–500 kW or more.

Mr. Bullen asks why panel heating is so expensive; it is largely a question of loading per square foot, ease of manufacture, and lack of popularity making bulk manufacture uneconomic. The necessary insulation at the back of the panel also raises the cost.

In reply to Mr. Whysall, Table A gives typical figures of oil-fuel consumption per kWh for ships' generating sets working at 220 volts. Particulars of the sets referred to in cols. (1), (2), and (3), are as follows: (1) Reciprocating steam-engine-driven sets with saturated steam at 250 lb. per sq. in. and 28 in. vacuum. (2) Turbo-generators with geared generators, steam being supplied at 400 lb. per sq. in. and 700° F. (3) Diesel-driven generating sets.

Table A

Oil-fuel Consumption in Pounds per Kilowatt-hour

kW	(1)	(2)	(3)
60	2·07	—	0·65
100	1·62	1·29	0·62
250	1·38	0·935	0·57
500	—	0·87	0·56
1 000	—	0·805	0·55

LONG FEEDERS FOR TRANSMITTING WIDE SIDE-BANDS, WITH REFERENCE TO THE ALEXANDRA PALACE AERIAL-FEEDER SYSTEM

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SUMMARY

The paper is concerned with reflection phenomena in long feeders used to transmit modulated high-frequency carrier waves such as television signals, in which the time of transmission of a wave along the feeder is comparable with the time periods of the modulation frequencies.

The nature of the various impedance irregularities giving rise to reflection is discussed, and their effect on a received television picture is indicated. Methods for reducing the effect of these irregularities are considered.

These reflecting irregularities may be studied in terms of the variation with frequency of the input impedance of the terminated feeder, and examples are given of the magnitude of the effects for various feeders.

The vision aerial feeder system of the London Television Station is described, including the aerial-to-feeder matching arrangements and a device for reducing the residual mismatch due to the variation of the aerial impedance over the frequency range of the carrier and side-bands.

Apparatus for measuring the input impedance and the methods adopted for establishing a standard resistance for terminating the feeder are described.

In the Appendix consideration is given to the permissible limits of eccentricity of an approximately terminated but irregularly eccentric feeder.

CONTENTS

* Electric and Musical Industries, Ltd.

CONTENTS—*continued*

(1) INTRODUCTION

This paper is concerned with the problems which arose in the course of the design of the aerial-feeder system for the Alexandra Palace Television Station erected in August, 1936.

In the preceding year complete vision and sound aerial and feeder systems were erected on a mast similar to that at Alexandra Palace at the E.M.I. Laboratories at Hayes for experimental purposes. In the course of these investigations it became apparent that there were stringent impedance-matching conditions to be satisfied if distortion of the transmitted picture was to be avoided. These conditions are connected with the fact that the time of travel of a wave along the feeder is not negligible compared with the time periods of the modulation frequencies. These conditions appear to have hitherto received little attention. The special requirements and the methods adopted to fulfil them are discussed in some detail.

Apparatus was designed to measure impedance at 45 Mc./sec. to an accuracy of a few per cent, and such measurements were fundamental to the design of transformers and impedance-correcting devices employed. The apparatus was further utilized to check the characteristics of the system at various stages of its construction.

At an early stage of the work it was necessary to produce a feeder-terminating resistance of the value of the characteristic impedance of the feeder which should be constant and non-reactive over a frequency range of from 43 to 47 Mc./sec. The difficulty in doing this will be appreciated when it is pointed out that at these frequencies the reactance of a lead consisting of an inch of wire may be of the order of 5 ohms.

The methods of measurement are described and curves are given which show the impedance of the aerial feeder system over the frequency range 42 to 48 Mc./sec. at important stages in the construction of the system.

The Alexandra Palace Television Station was designed to operate on a vision frequency of 45 Mc./sec. and a sound frequency of 41·5 Mc./sec. The pictures to be transmitted were 405 lines, 25 pictures per sec. with interlaced scanning, giving 50 frames per sec. This requires side-bands of approximately 2·5 Mc./sec.

The length of the feeder from the transmitter to the aerial is approximately 450 ft., so that the time of travel along it is 0·5 μsec., corresponding to a frequency in the side-band range.

(2) DISTORTION DUE TO REFLECTIONS IN THE FEEDER

A form of distortion of the transmitted picture with which this paper is particularly concerned is caused by reflected waves in the feeder due to the mismatch between the aerial and the feeder, and the transmitter and the feeder.

In order to obtain the wide side-bands required in the Alexandra Palace installation, it was necessary to arrange that the final output circuit of the transmitter should be heavily damped. The damping is due to the losses in the valves and circuits, together with the power

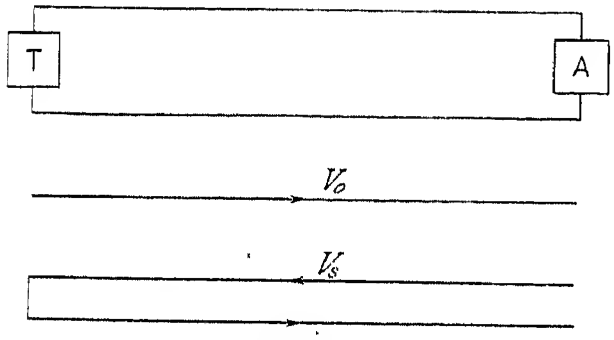


Fig. 1

supplied to the aerial. The damping due to the losses is small, and the major part is supplied by the aerial feeder system. This involves a large mismatch at the transmitter end of the feeder.

On account of the extremely low loss of the feeder and the high degree of mismatch at the transmitter end, a wave, if reflected from the aerial end, passes to the transmitter and arrives again at the aerial scarcely diminished in amplitude, but delayed by twice the time of travel along the feeder. This time-delay is about 1 μsec., during which time the scanning spot on a receiver screen will have travelled 1/100th of the width of the screen, a distance equal to that occupied by about 5 lines in the vertical direction. If the picture contains a thin vertical line it is obvious that reflections will give rise to distortion consisting of "echo" lines displaced from the original image at intervals of 1/100th of the picture width. A sharp edge would be followed by similar striae. Various forms of this distortion were observed in the early experiments at Hayes.

In order to estimate the accuracy of the termination required to avoid objectionable distortion, consider the transmission of a picture containing an abrupt transition from white to black, involving a sudden transition from full carrier to, say, zero carrier. If the final value is not zero the discussion is complicated by interference between the existing steady wave and the distorting waves to be described, but is otherwise unmodified.

Let the transmitter T (see Fig. 1) be of high impedance, so that substantially perfect reflection takes place at T of any wave arriving from the direction of the aerial A. During the transmission of the white portion of the

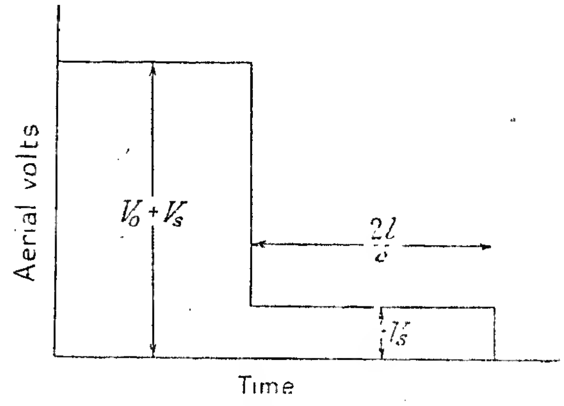


Fig. 2

picture the aerial current will be due to the resultant of the direct wave, of voltage V_0, initiated by the transmitter, and, if the aerial does not exactly terminate the feeder, a steady reflected wave V_s reflected from A and T in succession. Higher-order reflected waves will be neglected, since they must be of smaller magnitude than the first, and it will be shown that the first must be made negligible to avoid distortion. If l is the length of the feeder and c the velocity of the wave in the feeder, then at a time l/c after the stoppage of the transmitter the voltage on the aerial due to V_0 falls to zero, but that due to V_s persists a further time $2l/c$. This gives rise to a "step" in the decay of aerial current as indicated in Fig. 2. The relative height of the "step," if V_s is small, is given by the reflection coefficient

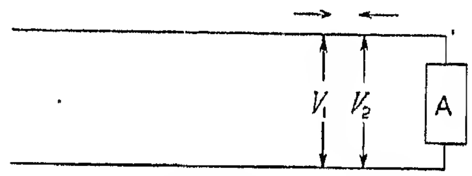


Fig. 3(*a*)

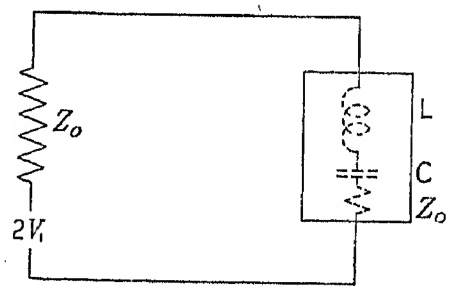


Fig. 3(*b*)

$(Z_a - Z_0)/(Z_a + Z_0)$, where Z_a and Z_0 are respectively the terminating and characteristic impedance of the feeder.

If the aerial is a resonant system then a further important factor must be taken into account. On the sudden decay of impressed volts from $(V_0 + V_s)$ to V_s the aerial current will not fall instantaneously to the new steady state but will decay with a period and damping determined by the constants of the aerial. This

effect would be much less important but for the fact that during the decay the aerial transfers part of its stored energy to the feeder, originating a wave which arrives back at the aerial after a delay of $2l/c$ and causes a further short burst of aerial current.

The magnitudes involved follow from the following considerations. If V_1 represents the voltage of the forward wave at the aerial end of the feeder (see Fig. 3a), and V_2 the reverse wave, then the voltage across the termination V_a equals $(V_1 + V_2)$. Further, the current and voltage across A due to the incident wave V_1 are equal to those which would be caused by an e.m.f. $2V_1$ in series with Z_0 and the aerial, as indicated in Fig. 3(b).

On the subsequent return of this wave, for which now $2V_1 = 2|V_0|e^{-\alpha t}\cos\omega_0(t - 2l/c)$, the transient aerial current i_t, calculated from the circuit of Fig. 3(b), is given,* neglecting the time-delay term, by

$$i_t = \frac{|V_0|te^{-\alpha t}}{L}\cos(\omega_0 t + \Psi) \quad . \quad . \quad . \quad (2)$$

The envelope is given by

$$\frac{|V_0|}{L}te^{-\alpha t} = \frac{|V_0|}{Z_0}\alpha te^{-\alpha t} = i_a\alpha te^{-\alpha t} \quad . \quad . \quad (3)$$

where i_a is the magnitude of the steady aerial current due to V_0.

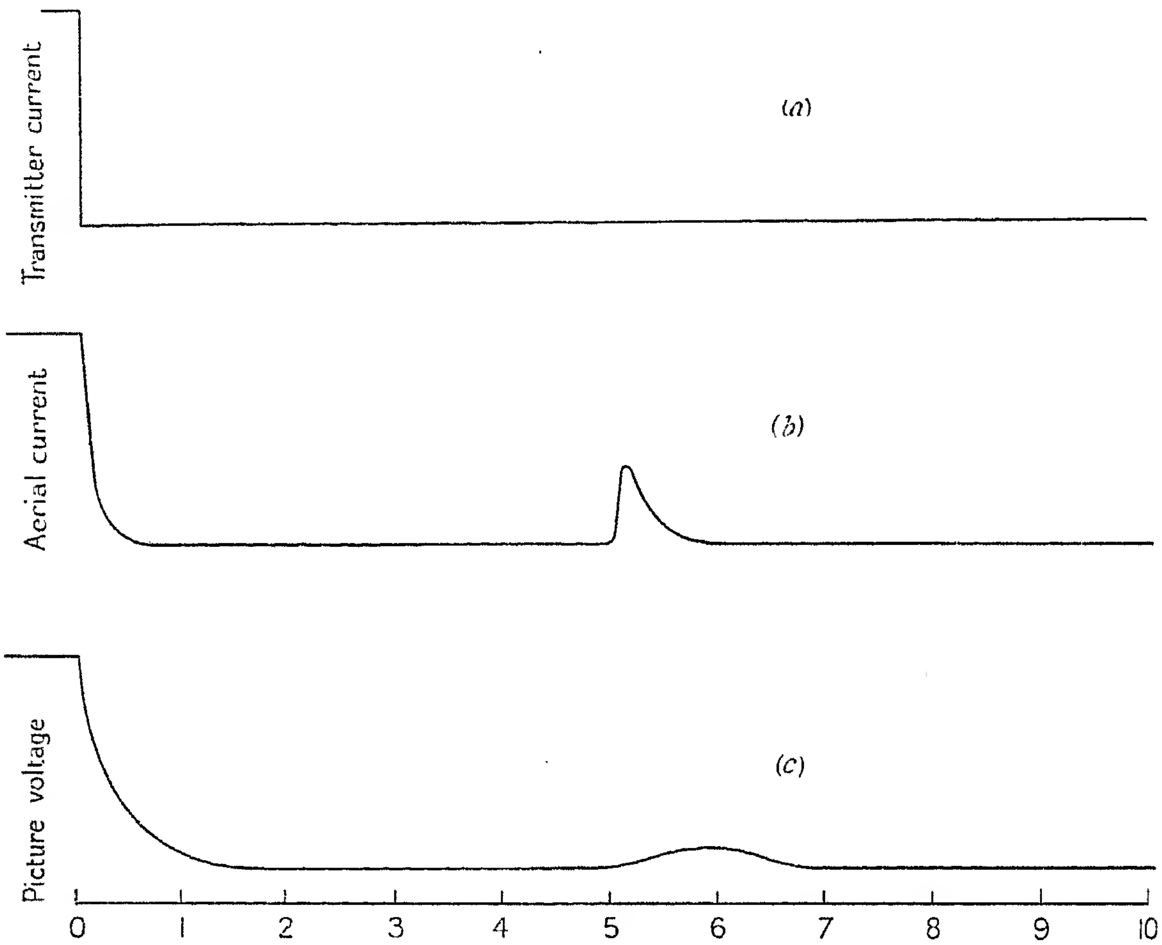


Fig. 4

(a) Transmitter current (envelope).
(b) Aerial current (envelope).
(c) Receiver picture wave-form.

As an illustration, consider A to be an inductance L in series with a capacitance C, tuned to the carrier frequency, and a resistance Z_0, so that there is no steady reflected wave V_s. Let the incident wave $V_1 = |V_0| \cos\omega t$ fall to zero at the aerial at time $t = 0$. The transient voltage across the aerial will be of the form

$$V_a = Ae^{-\alpha t}\cos(\omega_0 t + \phi) \quad . \quad . \quad . \quad (1)$$

where $$\omega_0 = \sqrt{\left[\frac{1}{LC} - \left(\frac{2Z_0}{2L}\right)^2\right]}$$

and $\alpha = \dfrac{2Z_0}{2L}$.

Also, from the initial conditions, $A = V_0$ and $\phi = 0$. Further, since after time $t = 0$, $V_1 = 0$, then $V_2 = V_a$ and the whole of this voltage is reflected.

The term $\alpha te^{-\alpha t}$, which determines the relative amplitude of the envelope of the burst of aerial current, reaches a maximum amplitude of $1/e$ when $t = 1/\alpha$, and then decays. The amplitude of this burst of aerial current is quite high and, moreover, not controllable. The duration, however, is very short and is a function of the aerial decrement α. Fig. 4 shows, in succession, (a) the envelope of the postulated transmitter current, and (b) the envelope of the aerial current as given by equation (3) for a half-wave dipole of $\frac{1}{16}$ in. diameter wire, tuned and matched at the carrier frequency and fed over a feeder 450 ft. in length. Such an aerial has a decrement α of about 40×10^6/sec.

The delayed burst of aerial current when acting on a receiver would tend to produce a picture voltage of

* See V. Bjerknes: *Wiedemann's Annalen*, 1895, vol. 55, p. 121.

reduced relative amplitude and extended duration on account of the selective circuits of the receiver. The effects would be the smaller the less the duration of the impulse. Fig. 4(*c*) shows in a qualitative manner the received picture wave-form on an idealized receiver. An estimate by Fourier methods of the amplitude of the disturbance in the picture wave-form due to the delayed burst of aerial current of Fig. 4(*b*), assuming an idealized receiver with a pass band of $\pm$ 2 Mc./sec., gave a value of about 10 % of the preceding steady voltage and indicated that for further increases in the decrement the amplitude would vary inversely as the decrement.

Though this analysis has considered the special case of a simple tuned circuit, a similar argument would appear to apply to other forms of resonant aerials.

To reduce the magnitude of the effect the aerial may be made to have a sufficiently high decrement by some means such as the employment of thick conductors, and also a certain amount of correction may be performed by inserting additional circuits which contribute transients in anti-phase to the original transient. A simple form of this latter arrangement in the case of the aerial circuit discussed would consist of a series-tuned

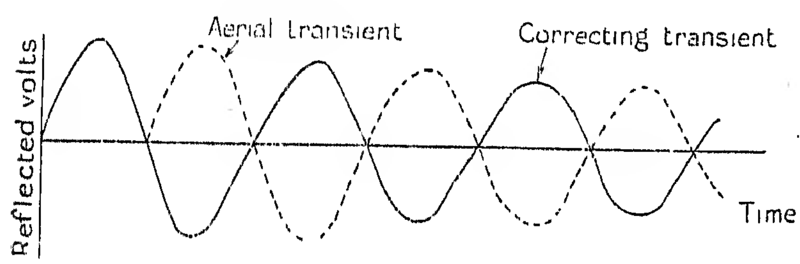


Fig. 5

circuit of the same L and C as the aerial, inserted in series with the feeder at one quarter-wavelength from the aerial. The resulting waves reflected down the feeder are indicated in Fig. 5, and it is apparent that the second circuit may effect a substantial degree of cancellation. It is obviously desirable that the two points of reflection in the feeder should be as close together as possible in order that as few half-cycles as possible may remain uncancelled. Beyond the stage at which the effect of the first transient is over before the arrival of the second no cancellation can be accomplished, but this limit to the distance which may separate the points of reflection is fixed not by the duration of the aerial transient itself, but by the duration of the effect on the final receiver circuit, since two completely isolated transients incident in succession on the receiver in appropriate phases may produce a resultant less than that due to either alone.

An alternative method of considering this phenomenon is in terms of the Fourier components of the signal. If the feeder is misterminated at any frequency within the range of the carrier and side-bands, the input impedance will vary from the characteristic impedance over this frequency range. Such a varying impedance is accompanied by a varying transfer of the Fourier components of the signal from the transmitter to the feeder. On account of the large mismatch at the transmitter, the energy transferred at one frequency will vary approximately linearly with the input impedance, and, since the loss in the feeder is negligible, the same energy is radiated from the aerial. These considerations indicate that it is a necessary condition for the avoidance of distortion that the input impedance to the feeder should be constant over the frequency range to be transmitted. This implies that the aerial should match the feeder over the whole of the frequency range, and, in addition, all causes of reflection in the feeder itself must be reduced to low values.

In order to estimate the tolerance which may be allowed in fulfilling these conditions without introducing appreciable distortion in the transmitted pictures, it is necessary to assign a value to the minimum change in signal which produces a discernible change of intensity in the received picture. This change of signal is dependent on the existing carrier level, and is very small in the neighbourhood of 30 % of the maximum, which in the Marconi-E.M.I. system is the picture-black level. In this region a change of carrier of the order of 2 % of the maximum can be observed.

Accepting this figure, it follows from the preceding discussion on a sudden transition in picture voltage from full white to black that the aerial must match the feeder at the carrier frequency to an accuracy of at least 2×2 % to avoid a visible step in the picture.

The permissible limit to the mismatch at the side-band frequencies is difficult to assign. In the case discussed of a resonant aerial matched at the carrier frequency (see Fig. 4) the distortion was 5 times that to be allowed. It would therefore appear that a decrement of not less than 5 times that of the case of Fig. 4, i.e. 200×10^6/sec., is desirable. A simple tuned circuit with this decrement would be mismatched by 12 % when 2 Mc./sec. off tune, and this figure is suggested as an estimate of the greatest unobjectionable mismatch throughout the frequency range 43 to 47 Mc./sec.

(3) FORMULAE FOR THE INPUT IMPEDANCE OF APPROXIMATELY MATCHED FEEDERS

(a) Localized Impedance Irregularities

In the course of the investigations the following approximate transmission-line theory was found very useful in reducing the labour of calculations and giving a clear picture of the processes involved.

It can be readily shown that the input impedance Z_i of a feeder of characteristic impedance Z_0 and of electrical length θ (Fig. 6), when terminated by an impedance $(Z_0 + a)$, is approximately given by

$$Z_i = Z_0 + ae^{-2\theta} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

This form is exact for small values of a and is accurate to 0·05 % of Z_0 for $|a|/Z_0 < 0 \cdot 1$. In the case of a feeder of negligible loss, such as the Alexandra Palace feeder, θ may be replaced by $j\alpha$ and the formula becomes

$$\begin{aligned} Z_i &= Z_0 + ae^{-2j\alpha} \\ &= Z_0 + |a|e^{j(\phi - 2\alpha)} \end{aligned}$$

where a is the complex quantity $|a|\underline{/\phi}$ and so may be written $|a|e^{j\phi}$. For an essentially air-spaced feeder $\alpha = 2\pi lf/c$, where l is the length of the feeder, c the velocity of light, and f the frequency. Now the quantity

$e^{-2j\alpha}$, which equals $\cos 2\alpha - j \sin 2\alpha$, is of unit magnitude and of phase angle -2α. Equation (4) may therefore be interpreted as follows. The input impedance is equal to the sum of the characteristic impedance Z_0 and the deviation of the terminating impedance from Z_0, the latter term being rotated through an angle -2α. This

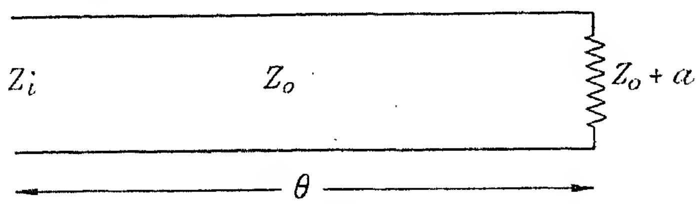


Fig. 6

rotation through an angle -2α is related to the wave reflected from the mistermination having to travel twice the length of the feeder in passing from the input to the termination and then back again.

The equation may be represented vectorially for any given values of Z_0, a, and α, as indicated in Fig. 7. Here OA represents the characteristic impedance and OB $= (Z_0 + a)$ is the terminating impedance. The input impedance OC is found by rotating the vector AB through the angle -2α to AC. The input resistance r_i and reactance x_i are given by OD and DC respectively, where D is the foot of the perpendicular from C on the line OA. The corresponding algebraic formulae are

$$\left.\begin{aligned} r_i &= Z_0 + a\cos(\phi - 2\alpha) \\ x_i &= a\sin(\phi - 2\alpha) \end{aligned}\right\} \quad . \quad . \quad . \quad (5)$$

Since $\alpha = 2\pi l f/c$ it follows that the input impedance is a function of the feeder length and frequency. For a fixed value of a the deviation of the input resistance from Z_0 has a sinusoidal variation with frequency of "periodic interval" equal to $c/(2l)$, and the reactance a similar variation in quadrature with the first. The word "periodic interval" is here used to denote the interval in the variable, in this case the frequency, between the successive points at which the function is in the same phase. In practice it is sufficient to measure the variation of resistance with frequency, since reactance provides no additional information.

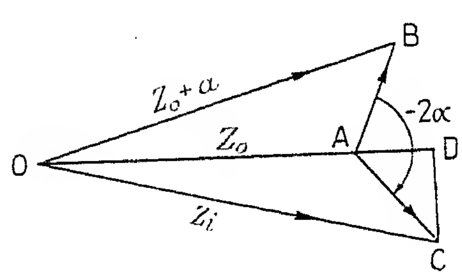


Fig. 7

Referring to Fig. 8 and applying equation (4) in succession to the points P and Q, it can be readily shown that the input impedance of a length of feeder terminated with the impedance $(Z_0 + a_1)$ and having an additional localized series impedance a_2 at a distance α_2, is given by

$$Z_i = Z_0 + a_1 e^{-2j\alpha_1} + a_2 e^{-2j\alpha_2} \quad . \quad . \quad (6)$$

Similarly for any number of localized series impedance irregularities the individual terms of the form $ae^{-2j\alpha}$ are additive, corresponding to addition of the individual reflected waves. The variation of the input resistance or reactance with frequency of a feeder with a number of fixed impedance irregularities is therefore given by the sum of a number of simple harmonic terms each of an

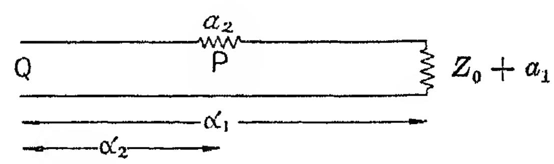


Fig. 8

amplitude dependent on the magnitude of the irregularity and of a "periodic interval" dependent on its distance from the input end of the feeder.

The discussion so far has been restricted to impedance irregularities which are in series with the feeder, such, for example, as could be caused by an imperfect contact at the joint between two sections. A common type of irregularity is an irregularity in the impedance between the two conductors, such as a localized decrease of impedance due to the presence of an insulator. Such cases are more readily treated in terms of admittances than of impedances. The basic formula for the input admittance Y_i, of a feeder of electrical length θ and of characteristic admittance Y_0, defined as $1/Z_0$, when terminated by an admittance $(Y_0 + A)$, is

$$Y_i = Y_0 + Ae^{-2\theta}$$

which is of the same form and subject to the same limitations as the impedance equation, equation (4). It follows that the derived equations for impedances and admittances are of similar forms. The latter type of irregularity may therefore be treated in terms of the admittance irregularity between the conductors of the feeder, or, alternatively, it is readily possible to transform the admittances to equivalent series impedances.

(b) Extended Impedance Irregularities

The effect of inserting a section of feeder of different characteristic impedance may be obtained by considering

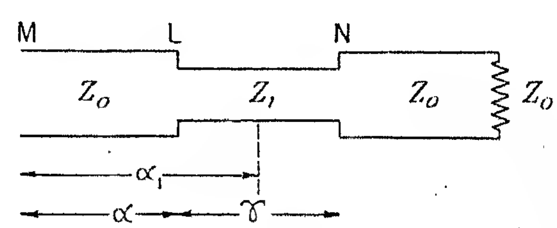


Fig. 9

the input impedance of the feeder shown in Fig. 9 at the points N, L, M, successively. By this means it can readily be shown that the input impedance at M is approximately given by:—

$$\begin{aligned} Z_i &= Z_0 + 2j\sin\gamma(Z_1 - Z_0)e^{-2j(\alpha + \frac{1}{2}\gamma)} \\ &= Z_0 + 2j\sin\gamma(Z_1 - Z_0)e^{-2j\alpha_1} \quad . \quad . \quad (7) \end{aligned}$$

where $\alpha_1 = \alpha + \frac{1}{2}\gamma$. This equation is identical with that

applicable to a localized impedance irregularity a inserted at the mid-point of the section of the feeder of abnormal characteristic impedance, provided that

$$a = 2j(Z_1 - Z_0)\sin\gamma$$

The irregularity gives rise to fluctuations of input impedance similar to those already discussed.

It is possible to reduce the effect of such a section of feeder either by making it short or by making it approximately a multiple half-wavelength in length so that $\sin\gamma = 0$. Alternatively, the effect may be cancelled by the insertion of a localized impedance irregularity such as a condenser across the feeder or a similar section of feeder an odd multiple of a quarter-wavelength from the centre of the abnormal section. Such a cancellation may hold sufficiently over the necessary frequency range if the spacing of the opposing irregularities is small.

(4) IMPEDANCE IRREGULARITIES IN THE FEEDER

(a) Insulators and other Capacitive Irregularities

In a completed aerial and feeder system the impedance at the transmitter is a function of the aerial impedance, together with any impedance irregularities which may occur along the length. Mechanical considerations require the provision of bends, joints, insulators, etc., any one of which may cause an impedance irregularity of appreciable magnitude.

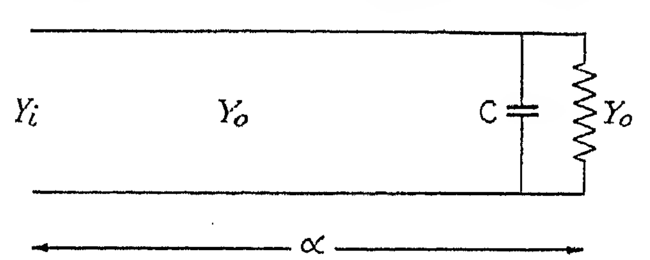


Fig. 10

Consider a single insulator in an otherwise uniform and correctly terminated feeder as shown in Fig. 10. If the insulator is substantially free from loss the admittance A introduced by its presence is equal to $j\omega C$, where C is the capacitance introduced and the input admittance is given by

$$Y_i = Y_0 + j\omega C e^{-2j\alpha}$$

and the conductance by

$$G_i = Y_0 + \omega C \sin\frac{4\pi lf}{c} \quad . \quad . \quad . \quad (8)$$

The insulator therefore produces a sinusoidal variation of G_i with frequency. In the case under consideration ωC is much less than Y_0, and the inverse of the conductance, i.e. the parallel resistance R_i, is given very nearly by

$$R_i = Z_0 - \omega C Z_0^2 \sin\frac{4\pi lf}{c} \quad . \quad . \quad . \quad (9)$$

The magnitude of C for various insulators considered ranged from 0·2 to 2 $\mu\mu$F, giving at 45 Mc./sec., for the type of feeder used, a fluctuation in R_i of from 0·5 % to 5 %, so that the effect of even a single insulator was large.

Equation (9) is important because it enables the magnitude and location of a capacitive impedance irregularity to be determined from measurements of the input resistance of the terminated feeder over a frequency range. Fig. 11 illustrates a possible curve of input resistance against frequency. Since it is sinusoidal it may be inferred that the impedance irregularities are restricted to a small length of feeder and so are probably due to a single cause. The "periodic interval" of the oscillation $(f_1 - f_2)$ locates the irregularity at a distance of $c/[2(f_2 - f_1)]$ from the input end, and an inspection of the feeder in this vicinity should show the cause. If this cause is assumed to be a local excess capacitance, which is common, it is possible to locate it accurately without precise frequency measurement by means of the phase of the resistance oscillation. Inspection of equation (9) shows that the frequencies at which R_i increases through the mean value are those for which the electrical lengths of the feeder to the point are odd multiples of a quarter-wavelength. Let the feeder be short-circuited in the vicinity of the capacitive irregularity, and the frequencies observed at which the short-circuited feeder is an odd multiple of a quarter-wavelength. If these frequencies are F_1 and F_2, etc. (see Fig. 11), then the electrical length to the short-circuited point is less than that to the irregularity by

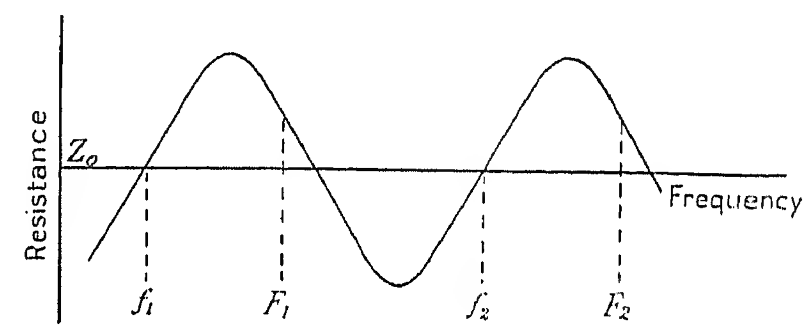


Fig. 11

$$\left(\frac{F_1 - f_1}{f_2 - f_1}\right)\frac{\lambda}{2} + \frac{n\lambda}{2}$$

where n is any integer. It is assumed that the measurement of the "periodic interval" is sufficiently accurate to determine n. Since it may be assumed that the velocity down the feeder is that of light, the physical position of the irregularity is thus located.

If it is not possible to remove the impedance irregularity, the effect may be substantially annulled by the insertion of an additional condenser of equivalent magnitude one quarter-wavelength distant from the point determined above. This method of cancellation was used when necessary at Alexandra Palace to reduce the variations in somewhat more complex curves than that of Fig. 11. The positions and magnitudes of the correcting condensers were determined by superposing curves such as that of Fig. 11 on the experimental curve and choosing an amplitude, frequency, and phase, to best effect cancellation. The type of condenser used consisted of a disc of brass on the end of a brass spindle screwed through the outer wall of the feeder. This disc introduced an excess capacitance between inner and outer, variable between about 0·2 and 1 $\mu\mu$F.

In the case of two insulators in an otherwise uniform feeder, there is a variation of the input conductance which is the sum of two sinusoidal terms; thus

$$G_i = Y_0 + \omega C_1 \sin \frac{4\pi l_1 f}{c} + \omega C_2 \sin \frac{4\pi l_2 f}{c} \quad . \quad (10)$$

In the particular case of two similar insulators, for which $C_1 = C_2$, the deviations from Y_0 cancel if $2\alpha_1 - 2\alpha_2 = \pi$, 3π, 5π, etc., i.e. if the insulators are separated by an odd multiple of a quarter-wavelength. Further, if the frequency changes over a small range the departure from this condition for a small spacing is not great and the cancellation is substantially maintained.

In general the variation of G_i with frequency indicated in equation (10) is that of the sum of two simple harmonic terms of "periodic interval" $c/(2l_1)$ and $c/(2l_2)$, respectively. This gives rise to a combined oscillation of fluctuating amplitude which is analogous to the well-known "beat" phenomena in sound. The maximum amplitude when the two are in phase is $(\omega C_1 + \omega C_2)$, and the minimum $(\omega C_1 - \omega C_2)$. The "periodic interval" of the combined oscillation is $c/(l_1 + l_2)$, and the interval between successive beat maxima is $c/[2(l_1 - l_2)]$, corresponding to the mean and the difference of the individual frequencies comprising the oscillations. In the same manner the susceptance shows similar phenomena displaced 90° in phase.

These forms of interference can, of course, occur equally well in the case of other forms of irregularities.

(b) The Experiment of Drawing Insulators Along a Concentric Feeder

To confirm the preceding theory and to obtain the order of magnitude of the resistance variations due to insulators, in the preliminary investigations experiments were made in which insulators were pulled along a length of feeder by means of a piece of string. The feeder was terminated at the far end with a resistance approximately equal to its characteristic impedance, and the input resistance was measured for various positions of the insulator. Fig. 12 shows the general arrangement, comprising a movable bell-type porcelain insulator between two fixed ones at the ends of the feeder. The input parallel resistance is shown in the graph plotted against l measured in wavelengths. The variation is approximately sinusoidal except near the ends of the feeder, in which positions the inner conductor sagged, so vitiating the results in these regions. The "periodic interval" of the resistance variation is approximately a half-wavelength, and the amplitude of the variation, 3·7 ohms, corresponds to a capacitance of 2 $\mu\mu$F. Low-frequency measurements (1 000 cycles per sec.) of the capacitance of a section of feeder with and without the insulator in position gave the same value. The mean value of the resistance variation is approximately the characteristic impedance of the feeder, but is modified by a number of constant factors such as the fixed insulators and the accuracy of the termination.

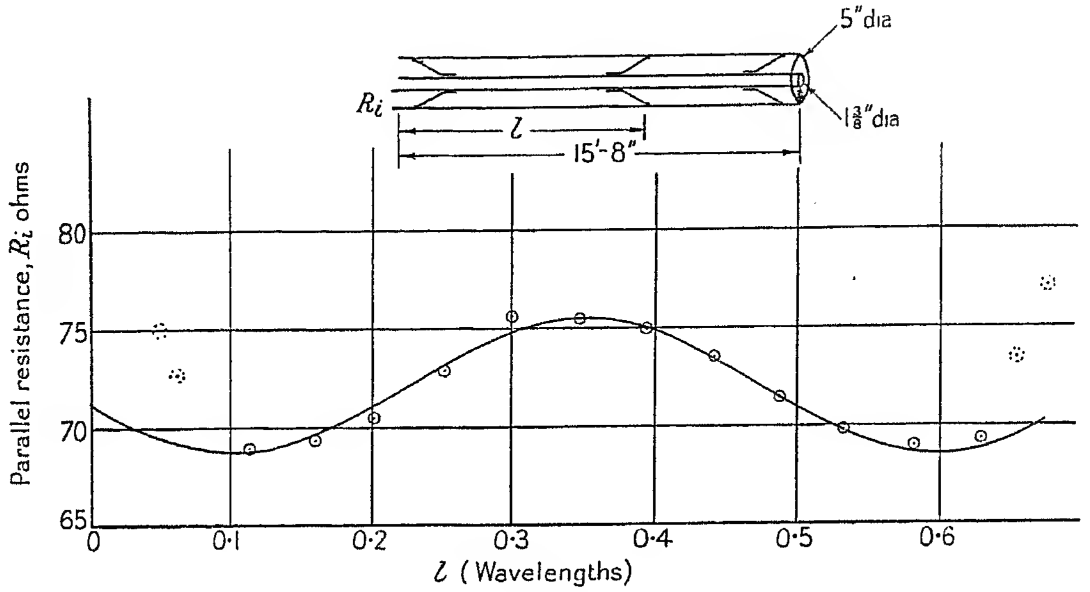


Fig. 12

A similar experiment was performed to investigate the cancelling effect of two insulators at a quarter-wavelength spacing. The arrangement is shown, together with the measurements of the resistance, in Fig. 13. The curve shows that the cancellation is complete to 0·5 ohm, which was about the accuracy of the measurement in these preliminary experiments.

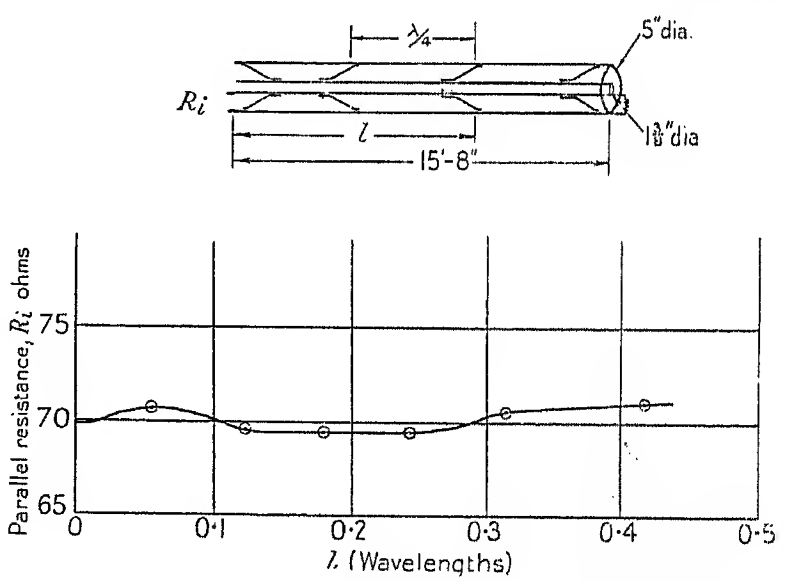


Fig. 13

A variation of this method was adopted for measuring the capacitance introduced by the various types of insulators. A comparatively short length of terminated feeder was used and the input resistance measured. The feeder was then taken to pieces, one or more insulators were inserted in the position corresponding to the first minimum of Fig. 12, and the feeder was then re-assembled. The change of the input resistance was then a measure of the capacitance change.

(c) Uniform Spacing of Insulators

The preceding Section has indicated that even a single insulator may give rise to large variations of input impedance, but that it is possible that insulators may be so arranged that their effects cancel. A particular case of considerable importance is that of a feeder having uniformly spaced insulators. The previous analysis neglected the second-order terms due to the interaction between the insulators, and its validity is doubtful for a large number of insulators when the sum of the first-order terms is nearly zero.

An exact analysis of this case may be obtained by considering this loaded feeder replaced by a new feeder of modified constants to which it is equivalent at a given frequency. The loaded feeder may be broken up into π sections at the middle of each insulator, the admittance of the insulators added to the parallel members, and the constants of the equivalent feeder sections obtained. In this way it may be shown that if:—

Z_0 = characteristic impedance of the feeder without insulators,
Z_0' = characteristic impedance of the feeder with insulators,
l = insulator spacing,
λ = wavelength in the unloaded feeder,
λ_1 = wavelength in the loaded feeder,
θ = electrical length of a section of unloaded feeder,
θ_1 = electrical length of a section of loaded feeder,
$\alpha = \frac{2\pi l}{\lambda}$,
$\alpha_1 = \frac{2\pi l}{\lambda_1}$,
C = capacitance of each insulator,
$\frac{1}{Z_a}$ = admittance change due to the presence of an insulator,

then
$$Z_0' = \frac{Z_0}{\left[1+\left(\frac{Z_0}{2Z_a}\right)^2+\frac{Z_0}{Z_a}\coth\theta\right]^{\frac{1}{2}}} \quad . \quad (12)$$

$$\sinh\frac{\theta_1}{2} = \sinh\frac{\theta}{2}\left(1+\frac{Z_0}{2Z_a}\coth\frac{\theta}{2}\right)^{\frac{1}{2}} \quad . \quad . \quad (13)$$

It may be assumed that the losses in the feeder and insulators are negligible, so that

$$\theta = j\alpha \quad \text{and} \quad \theta_1 = j\alpha_1, Z_a = \frac{-j}{C\omega}$$

and also that $\left(\frac{Z_0}{2Z_a}\right)^2$ is negligible, so that

$$Z_0' = \frac{Z_0}{(1+Z_0C\omega\cot\alpha)^{\frac{1}{2}}} \quad . \quad . \quad . \quad (14)$$

and

$$\cos\alpha_1 = \cos\alpha - \tfrac{1}{2}Z_0C\omega\sin\alpha \quad . \quad . \quad (15)$$

It is evident that the constants of the equivalent feeder are a function of frequency, and hence over a range it would require a terminating resistance varying with frequency in order to avoid reflections.

The special case of a quarter-wavelength spacing of insulators is of interest in that the characteristic impedance of the feeder is unmodified by the presence of the insulators. If the spacing of the insulators is in the neighbourhood of a half-wavelength the rate of change of the constants of the equivalent feeder is very rapid, and this condition is to be avoided. At this precise spacing the characteristic impedance goes to zero, which is associated with the whole capacitance of the insulators numerically adding.

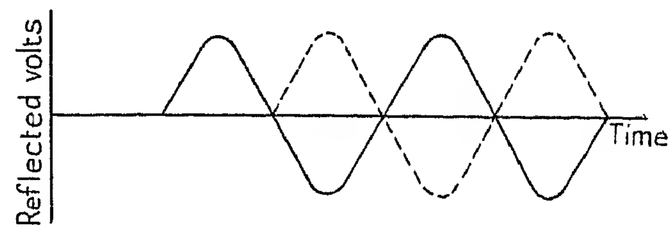


Fig. 14

An alternative method of considering the problem is in terms of a suddenly applied impressed sinusoidal voltage given by $e^{j\omega t}1$ in the notation of the operational calculus, applied to the feeder terminated by a condenser in parallel with its characteristic impedance. The reflected voltages arising are

$$-\frac{jZ_0C\omega e^{j\omega t}}{2+jZ_0c\omega} - \frac{2e^{-\frac{2t}{Z_0c}}}{2+j\omega Z_0c} \quad . \quad . \quad . \quad (16)$$

of which the second term is a transient of negligibly short duration and the first is a steady reflection. A second insulator placed farther down the feeder will give rise to a similar steady reflected wave, and if the insulators are spaced at $\frac{1}{4}$ wavelength the second wave will be delayed by a half-period and so these effects will cancel except for the first half-cycle. This is shown in Fig. 14, in which the full and dotted lines show the voltage waves reflected from the first and second insulators respectively. Hence if the insulators are spaced along the length of the feeder at $\frac{1}{4}$-wavelength intervals the resultant reflected wave will consist of a series of half-cycles as indicated in Fig. 15, which assumes an even number of insulators. The wave-form of Fig. 15 will persist for a time equal to the time of travel of the wave over twice the length of the feeder.

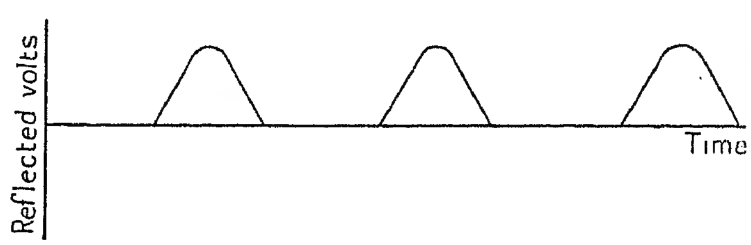


Fig. 15

The effect on the receiver is therefore to produce a step in the picture of this duration and of amplitude dependent on the amplitude of the carrier-frequency component of the wave of Fig. 15, in this case obviously about one-half that due to a single insulator. The relative amplitude of the wave reflected by an insulator of capacitance 1 $\mu\mu$F in an 80-Ω feeder at 45 Mc./sec. is $1 \cdot 1$ %, so that with the above spacing a step in the picture of $0 \cdot 5$ % amplitude would occur.

At closer spacings the cancellation of the carrier-frequency component is more complete. The spacing of half-wavelength is particularly objectionable in that the whole of the reflected waves add in phase.

If the spacing is not uniform there is a probability of considerably less perfect cancellation. For example, for completely random spacing of insulators the probable value of the amplitude of the reflected wave, corresponding to Fig. 15, would be $a\sqrt{N}$, where N is the number of insulators from which waves are arriving at any moment, and a the amplitude due to a single insulator.

The practical conclusion to be drawn from this discussion is that insulators of low capacitance should be used, spaced at small equal intervals.

(d) Bends and Expansion Joints

A difficulty in the construction of an ideal uniform feeder is that it is required to change its direction at certain points. At such bends considerable mechanical support of the inner is required to hold it in position, and it is usually necessary to provide additional insulators for this purpose. Such additional insulators give rise to irregularities of impedance. If the bend is in the form of an arc of a circle, a large number of insulators will be required and will produce a large impedance irregularity. This effect may be cancelled by such means as reducing the relative size of the inner at the bend so that with the additional capacitance of the insulators the characteristic impedance of the section remains unaltered. Alternatively, it is possible to cancel the effect of a group of insulators over a considerable range of frequency by a second similar group individually spaced $\frac{1}{4}$ wavelength away.

In the Alexandra Palace installation right-angle bends were adopted which permitted the use of angle boxes with a single large conical insulator for locating the inner at the bend. The boxes were so proportioned as to give a deficiency of capacitance at the box, which was partially compensated by the capacitance introduced by the insulator, and the residue was taken up by a simple variable plate condenser adjusted to the desired value.

In the construction of long lengths of feeder, expansion joints are provided in both the inner and outer conductors to allow for expansion with temperature-changes. These joints are frequently in the form of outer sleeves which are a sliding fit over the outer and inner of the feeder. Assuming an outer diameter of 5 in. and an inner of $1\frac{3}{8}$ in. with sleeves of $\frac{1}{32}$ in. wall, the change of the characteristic impedance is $4 \cdot 5\ \Omega$, which, for an expansion joint of length 5 ft. 6 in., gives the large amplitude of $\pm$ 9 ohms for the input-resistance variation. This fluctuation is due to the proportionally large variation of the inner diameter.

To reduce this cause of irregularity in the Alexandra Palace system, the sleeves over the inner were replaced by pipes sliding within the normal inner, which permitted the minimum length of abnormal-diameter section. The joints were located at the angle boxes so that the residual error of the joint could be compensated by the variable condenser located in the box.

(e) Eccentricity of the Feeder Conductors

The preceding Sections have described localized irregularities introduced into an otherwise uniform feeder. This uniform feeder itself is not realizable in practice on account of variations in the diameters, shapes, and degree of eccentricity, of the conductors. Of these factors eccentricity appears to be the most important in the Alexandra Palace type of feeder. The insulators were arranged to be sufficiently loosely fitting to permit the relative expansion of the inner conductor, and, since the conductors tend to be somewhat irregular in cross-section and the insulators must be sufficiently loose not to jam in the tightest parts, there is often a considerable amount of tolerance in the location of the inner conductor. As a result of this and the spacing between insulators, the inner conductor tends to be slightly bowed and eccentric in an irregular manner. This eccentricity produces impedance irregularities similar to those already discussed, but which, on account of their random nature, are much more difficult to reduce.

In the Alexandra Palace installation the other sources of irregularity were reduced to small values by the means already discussed, so that the eccentricity remained as the outstanding cause of irregularities in the feeder. It was found that, by rotating sections of the inner conductor, the input impedance could sometimes be changed by an ohm or so. To reduce the effect of eccentricity, the inner was rotated to a best position and then, if necessary, condensers were inserted at selected points along the feeder to produce counteracting irregularities in the manner previously discussed. Also the condensers in the angle boxes were used for this purpose.

Obviously it is very undesirable that this form of correction should be necessary, and, in an Appendix, an estimate is made of the tolerance which could be allowed in centring the inner conductor without excessive impedance irregularities arising. It is there concluded that, for a feeder similar to that at Alexandra Palace, if the displacement of the inner conductor from the centre could not exceed $0 \cdot 05$ in. the probable variation of input impedance due to eccentricity would be $\pm$ 1Ω and the greatest possible value could not exceed 5 ohms.

(f) Examples of the Variation of Input Impedance with Frequency of Matched Feeders

In this Section a few examples are given, illustrating the behaviour of actual feeders.

Fig. 16 shows an early measurement of the variation of the input resistance of a terminated feeder of length 152 ft. The feeder consisted of concentric copper pipes $3\frac{1}{4}$ in. and $\frac{7}{8}$ in. outer and inner diameters respectively, with porcelain bell-type insulators. It consisted of a straight portion of 116 ft. followed by three bends in the

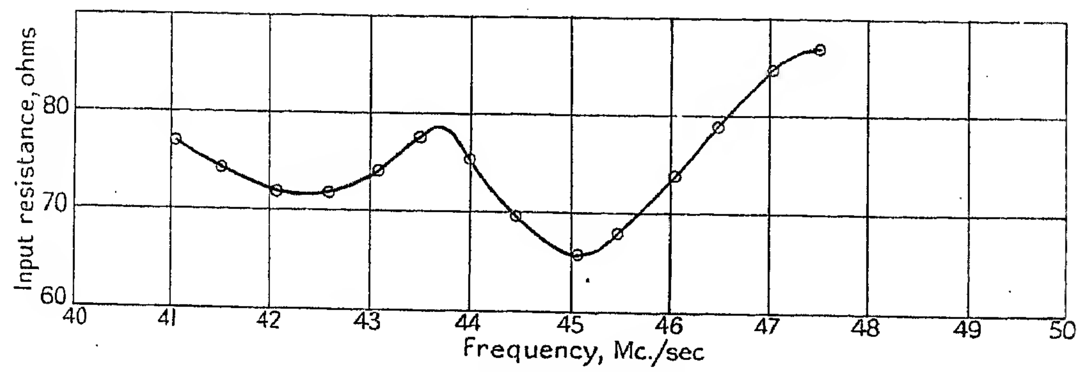


Fig. 16.—152-ft. length of $3\frac{1}{4}$ in. feeder with three right-angle bends.

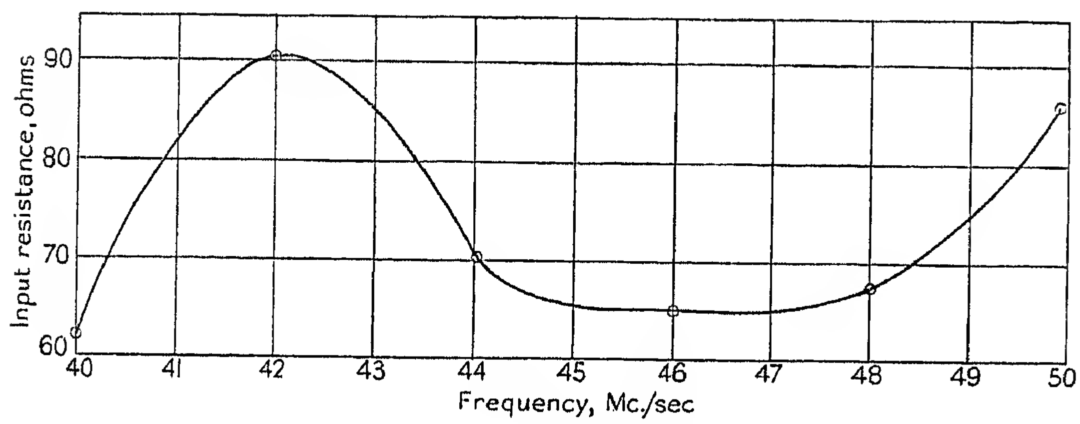


Fig. 17.—Straight 117-ft. length of $3\frac{1}{4}$ in. feeder with " bell " type insulators irregularly spaced.

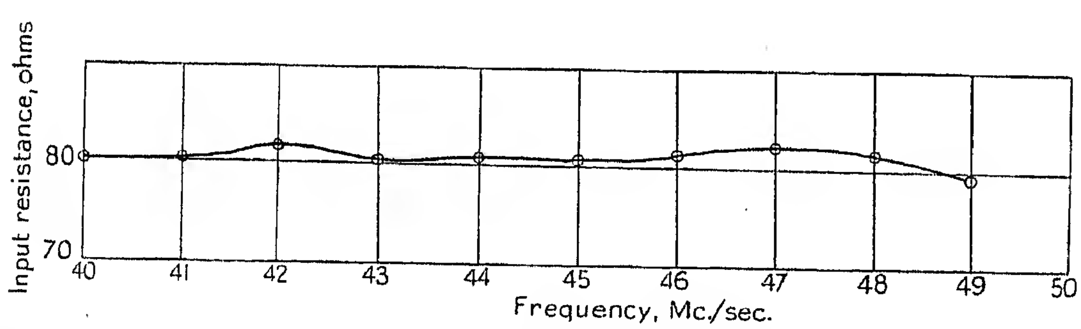


Fig. 18.—Straight 97-ft. length of 5-in. Alexandra Palace type feeder with steatite rod insulators at $\frac{1}{8}\lambda$ intervals.

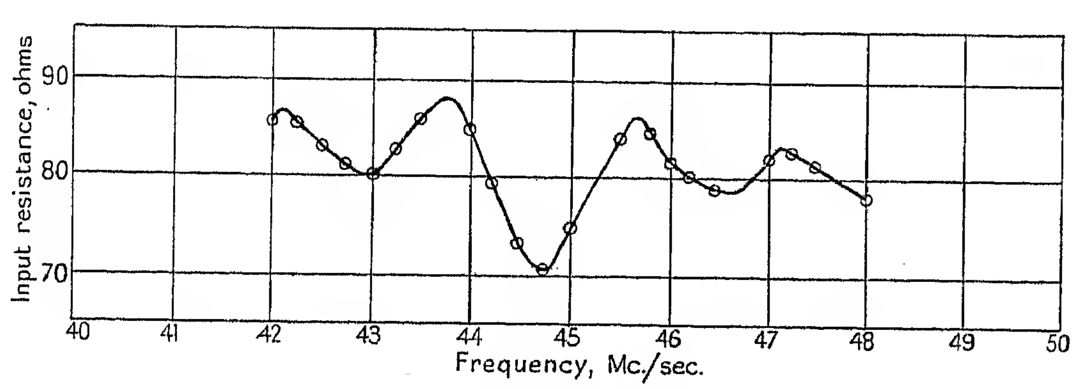


Fig. 19.—320 ft. of 5-in. Alexandra Palace type feeder without impedance correction.

form of arcs of circles and straight connecting portions. Insulators had been concentrated at each bend for mechanical reasons. The curve shows a fluctuation of resistance of considerable magnitude, suggesting a sine wave of increasing amplitude such as that given by two separated point disturbances.

Fig. 17 shows an input-resistance measurement on a similar straight section of feeder of length 117 ft. as originally laid for a sound transmitter in which no special precautions had been taken as to the spacing of the insulators.

Fig. 18 shows the results of the first 97-ft. trial section of feeder of similar type to that laid at Alexandra Palace. In this case low-capacitance insulators were used, spaced rigorously at equal intervals of about $\frac{1}{8}$ wavelength. The curve shows that the fluctuation is much reduced and of a periodicity that suggests a slight mistermination at the end of the feeder.

The Hayes vision feeder was laid with due attention to all the errors mentioned in the previous Sections, and the input impedance characteristic was measured as each additional section of the feeder was completed. Before proceeding to the next section of the feeder the variation of impedance was reduced to a small value by the adjustment of angle-box condensers, and in some cases by the insertion of additional condensers at appropriate places. As a result of this procedure the overall impedance characteristic varied less than $\frac{1}{2}$ ohm from the mean over the frequency range 41·5–47·5 Mc./sec. A feeder to the sound aerial was also in the course of construction and, as an experiment, it was decided to lay it as nearly as possible in the same manner as the vision feeder, except that no attempt was made to adjust the impedance characteristics of the individual sections in the course of construction. When completed an overall curve of impedance against frequency was taken. The result is shown in Fig. 19. The overall variation of resistance is large, being 17 ohms. The curve is irregular, and the obviously large component of "periodic interval" about 1·5 Mc./sec. indicates that there is a serious irregularity near the end.

It was concluded that it would be necessary for the vision system at Alexandra Palace to adjust individually each section of the feeder as had been done at Hayes.

(5) MATCHING THE AERIAL TO THE FEEDER

(a) The Aerial Transformer

The problem of matching the aerial, which is a resonant system whose impedance varies with frequency, to the feeder presents greater difficulty than those so far discussed. If the aerial is matched at the mid-frequency the deviations of impedance may cause a serious mismatch at the side-band frequencies.

In the Alexandra Palace system a transformer was provided to transform the aerial impedance to that of the feeder at the mid-frequency, and means were also provided to cancel partially these residual mismatches. Measurements of the aerial impedance, which was complex and about one-quarter of the characteristic impedance of the feeder, showed that both the resistive and reactive components varied with frequency.

Considerations of the possibility of simultaneously annulling the reactance and matching to the feeder at the mid-frequency led to the adoption of a transformer section of feeder, the principle of which is described below. Consider the arrangement in Fig. 20, where $Z_i = R + jX$ is the input impedance when the feeder is terminated by the impedance $Z_a = r + jx$. Then

$$Z_i \doteq Z_0\left\{\frac{Z_a \cos\alpha + jZ_0 \sin\alpha}{Z_0 \cos\alpha + jZ_a \sin\alpha}\right\} \quad . \quad . \quad (17)$$

Separating equation (17) into its real and imaginary components and eliminating α, it can be shown that

$$R^2 + X^2 + Z_0^2 - R\frac{r^2 + x^2 + Z_0^2}{r} = 0 \quad . \quad (18)$$

Assuming, therefore, that we desire to transform to a given pure resistance R, we obtain, on putting $X = 0$,

$$Z_0^2 = Rr\left\{1 - \frac{x^2}{r(R - r)}\right\} \quad . \quad . \quad . \quad (19)$$

giving the required value for Z_0. We also obtain, by substitution in equation (17),

$$\tan 2\alpha = \frac{-2xZ_0}{Z_0^2 - r^2 - x^2} \quad . \quad . \quad . \quad (20)$$

We have therefore determined the constants of a feeder which, when connected in series with the aerial,

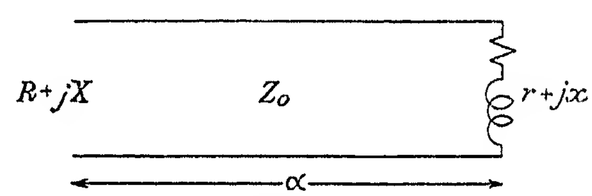


Fig. 20

transforms its impedance at one frequency to a given pure resistance. The operation of the feeder is somewhat similar to that of a $\frac{1}{4}$-wavelength transformer, transforming from a low resistance r to a high resistance R. Owing to the fact that it is terminated by a resistance with a small positive reactance, there is a length of feeder slightly shorter than a $\frac{1}{4}$ wavelength for which the input impedance is resistive. Further, because it is a considerably mismatched feeder it would, if terminated with a constant impedance, show a varying input reactance over the frequency range. This variation, in the case of the Alexandra Palace aerial, opposes that due to the change of the aerial reactance. In consequence the device tends to cancel the overall reactance of the aerial and to leave a substantially pure but varying resistance. For this reason, and on account of its simplicity, it was decided to make use of this type of transformer.

(b) Reduction of the Residual Mismatch

Further to reduce the effect of the residual reflections at the side-band frequencies due to the variation of impedance with frequency after transformation, a method was adopted of deliberately introducing an irregularity at a chosen point in the feeder which would contribute a reflected wave in anti-phase to that due to the residual mismatch. The choice of the point of

insertion and the nature of the irregularity to be inserted depends on the following considerations. To avoid loss of power it is desirable that the inserted irregularity should be purely reactive. Further, it is necessary that the inserted irregularity should have no effect at the frequency at which the aerial is matched to the feeder. In consequence a tuned circuit of low loss suggests itself as the possible form to be adopted. A parallel tuned circuit aross the line would have a positive reactance at frequencies below the resonance, and vice versa. If, therefore, a point on the feeder exists at which the parallel resistance of the feeder terminated by the aerial-transformer section is constant, accompanied by a parallel reactance equal but opposite in sign to that of a tuned circuit, the connection of the tuned circuit across this point would cancel the reactance and leave a constant resistance. In this method deviations of the aerial impedance are transformed by the feeder to variations of reactance, which are then neutralized without loss of power by a tuned circuit of appropriate selectivity.

Consider a feeder, matched to the aerial at a frequency f_0, the characteristic admittance of which is represented by the vector OA of Fig. 21. Let the deviation of aerial admittance at a frequency f_1 be represented by the

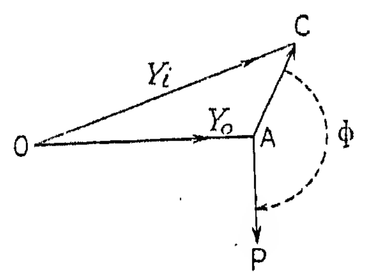


Fig. 21

line AC, then a length α of feeder can be chosen for which the input conductance is equal to Y_0 and the susceptance is negative, being given by AP (see Fig. 21). This requires that $2\alpha = 2n\pi + \phi$, where n is any integer and ϕ is indicated in the Figure. If f_1 is greater than f_0 a tuned circuit resonant to f_0 can be found having a positive susceptance at f_1 of magnitude AP. In a similar manner, for a frequency f_2 equally below f_0 another series of lengths of feeder exist, having a conductance equal to Y_0 and a positive susceptance. This susceptance could be neutralized by the same tuned circuit if the deviation of admittance from Y_0 were equal to that at f_1. Since the electrical length of the feeder varies with frequency it is usually possible to find a physical length of feeder at the end of which a suitable tuned circuit would annul the susceptance at both frequencies.

As an illustration of the process, consider a case in which at both frequencies f_1 and f_2 the conductances are equal and lower than the characteristic conductance of the feeder, and the susceptances zero. In Fig. 22, OA represents the characteristic admittance of the feeder, and AC_1 and AC_2 are the deviations of the terminating conductance at frequencies f_1 and f_2 respectively. Assuming f_1 to be the higher frequency, it is required to find a physical length of feeder such that AC_1 rotates to AP_1 while AC_2 rotates to AP_2. It is seen that AC_1 must rotate through 180 degrees more than AC_2. This corresponds to the electrical length of the feeder being $\frac{1}{4}$ wavelength greater at the higher frequency. An alternative conception is that if waves of frequencies f_1 and f_2 are applied to the required length of feeder the waves reflected from the end at frequencies f_1 and f_2

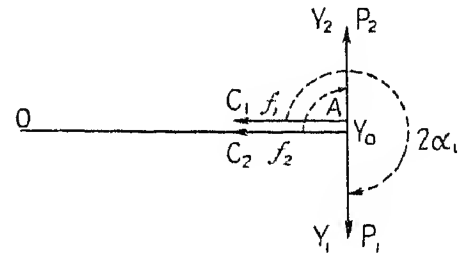


Fig. 22

must arrive in anti-phase. In this particular case, since the reflecting impedances are identical the electrical length of the feeder at the two frequencies must differ by $\frac{1}{4}$ wavelength. This length is given by

$$l = \frac{\lambda_0}{4} \cdot \frac{f_0}{f_1 - f_2}$$

If $f_0 = 45$ Mc./sec. and $(f_1 - f_2) = 4$ Mc./sec., $l = \frac{1}{4}(11 \cdot 25\lambda)$. For this length P_1P_2 is not at right angles to OA but the nearest value to obtain this relationship is 11 quarter-wavelengths, and for this length AP_1 and AP_2 are still substantially in anti-phase.

Let a parallel tuned circuit be connected across the feeder so as to tune at f_0 and of such selectivity as to have susceptances $- AP_1$ and $- AP_2$ at frequencies f_1 and f_2 respectively. A convenient form for such a circuit consists of a $\frac{1}{4}$-wavelength of feeder short-circuited at one end and tapped at an intermediate point AB as shown in Fig. 23. For this circuit the rate of change of

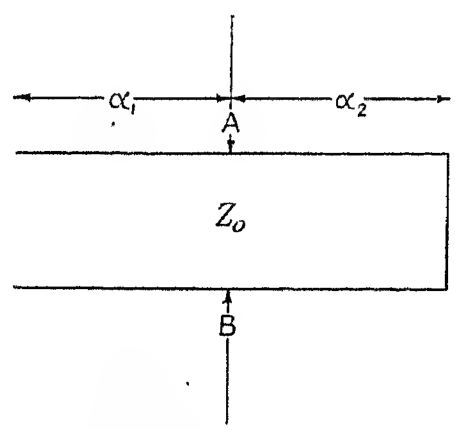


Fig. 23

admittance with frequency from the tuned value is given by

$$\frac{dY}{df} = j\,\frac{\pi \sec^2 \alpha_1}{2fZ_0} \quad . \quad . \quad . \quad . \quad (21)$$

so that for a given selectivity and Z_0 the length α_1 may be determined.

From the above it is evidently possible to find a tuned circuit which when placed across the feeder at a selected point will reduce to zero, at two frequencies as well as at the matched frequency, the variation due to the

varying aerial impedance. For other frequencies in the range the cancellation may not be perfect, but for simple types of variations of the aerial impedance it will be found possible to reduce substantially the overall fluctuations by this means.

(6) IMPEDANCE-MEASURING TECHNIQUE

(a) Measuring Apparatus

Special forms of impedance-measuring gear were devised, since at these frequencies considerable difficulty is experienced with the appreciable reactance of even the shortest leads, the large effect of small stray capacitances, and the lack of reliable standard resistances. One form was self-contained and portable and capable of measuring resistance and reactance over a wide range of values of impedance and frequency. A second form was designed to attach to the end of the feeder, and was capable of measuring small deviations of resistance from a fixed value.

In both forms the impedance to be measured is placed in parallel across a tuned circuit which is coupled to an oscillator and has a diode voltmeter across the condenser. The circuit is tuned by means of the condenser, and the diode volts are noted. A resistance is then substituted in place of the unknown impedance of such a value as to give the same diode volts when the circuit is retuned. The resistance is equal to the parallel resistance of the impedance, and the change in capacitance of the resonant circuit measures the parallel reactance. The apparatus is enclosed in a copper case to which one side of the tuned circuit is connected.

The portable gear (see Fig. 24*a*) comprised a variable-frequency oscillator followed by an amplifying and isolating stage, the anode of which was tuned. To this tuned circuit the measuring circuit was inductively coupled, provision being made to switch coils in parallel with the measuring circuit in order to change the point of tuning on the condenser. An Isolantite low-loss 150-$\mu\mu$F condenser calibrated at low frequencies was used as the measuring condenser. A method of measuring the residual inductance was devised, so that the equivalent values of the capacitance at high frequencies were known. The method consisted in noting the apparent value of a small fixed condenser placed in the measuring cups at various points over the range of the variable condenser. The condenser setting for resonance was varied by placing other fixed reactances in parallel across the tuned circuit. From the variation of these apparent values the effective inductance of the condenser was calculated and the equivalent capacitances at high frequencies were deduced. This method has since been described by Field and Sinclair.* Across the tuned circuit a diode voltmeter of variable sensitivity was connected. Mercury cups were provided as terminals, and leads to the case and the terminals were made as thick and short as possible. The apparatus was mains-operated.

The feeder-measuring gear (see Fig. 24*b*) was adapted for measuring accurately small changes in the feeder input resistance. The layout of the measuring tuned circuit was modified to reduce the length of feeder connecting leads to a minimum. The inductance of the tuned measuring circuit was formed of a pick-up coil and a suitable length of screened connecting cable so that the oscillator could be removed to a distance of a few feet. A box containing the measuring condenser, the diode, and its associated circuits, was clamped on to the end of the feeder and so arranged that a short sleeve over the inner of the feeder could be slipped down into a mercury pool. A switch of the rocking type was provided, connected to the live terminal of the condenser and dipping either into the mercury pool or into a second pool, provided to permit the bridging of a suitable resistance to the case. This switch, on account of its finite inductance, introduced a small error into the measured impedance so that this gear could only be used for the comparison of substantially equal resistances. The diode voltmeter was arranged as before but with the addition of a circuit for backing off the steady d.c. diode

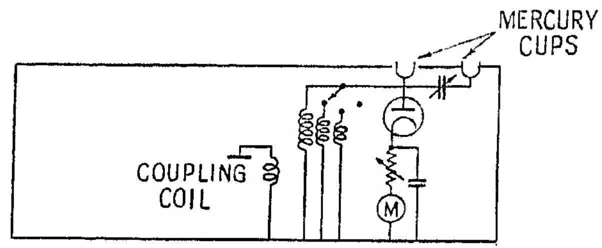


Fig. 24(*a*).—Portable impedance gear.

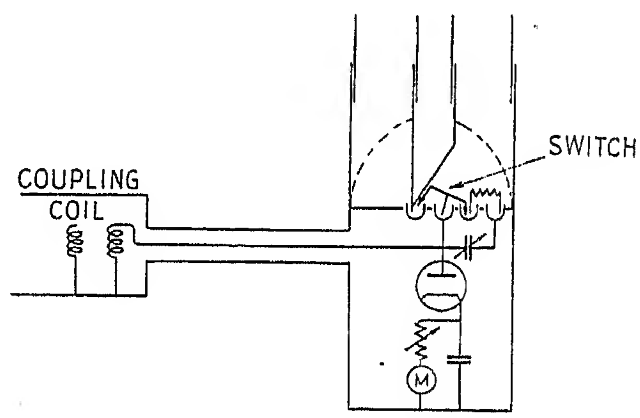


Fig. 24(*b*).—Feeder-measuring gear.

current and the provision of a suitable galvanometer to obtain enhanced sensitivity.

The accuracy of the frequency calibration is of considerable importance as the measured variation of resistance with frequency has a period of the order of 1 Mc./sec. If, therefore, we wish to insert an irregularity to compensate for an observed error, it is necessary to know the frequency to an accuracy of about 0·05 Mc./sec., in order accurately to locate the point of insertion from the phase of the oscillation of the input resistance. The oscillator was arranged to have a very open scale, and the calibration was obtained from the transmitter frequency (45 Mc./sec.) and by short-circuiting the feeder at a distant point and measuring the $\frac{1}{4}$-wavelength frequencies which gave known frequency-differences.

Fixed resistors of the Dubilier half-watt metallized type were used, and intermediate values were obtained by interpolation and by the use of parallel combinations. The d.c. values were measured after use and were

* *Proceedings of the Institute of Radio Engineers*, 1936, vol. 24, p. 255.

accepted as correct at high frequencies. This procedure was justified by the fact that the characteristic impedance of the terminated feeder came to the theoretical value when measured in this way.

Using the portable gear, measurements of resistance of the order of 80 Ω could be reproduced to about 0·5 Ω with an absolute accuracy of about 1 or 2 Ω, and measurements of capacitance to about 1 $\mu\mu$F. This capacitance corresponds to a parallel reactance of 3 600 Ω or a series reactance of 1·7 Ω. The feeder-measuring gear was adjusted to give small differences in resistance correct to 0·1 Ω, and the capacitance change was not recorded.

(b) Feeder-Terminating Resistors

Terminating resistors were formed of short sections of line (as shown in Fig. 25) which plugged on to the end of the feeder. A number of half-watt resistors arranged in parallel were connected by short leads across the end of the feeder, the residual inductance of the leads being cancelled by a small variable condenser C. The inner of the feeder was located by means of insulator rods passing through the inner and outer. It was necessary to adjust the resistor and condenser, so that in the presence of the rod insulators the input impedance across

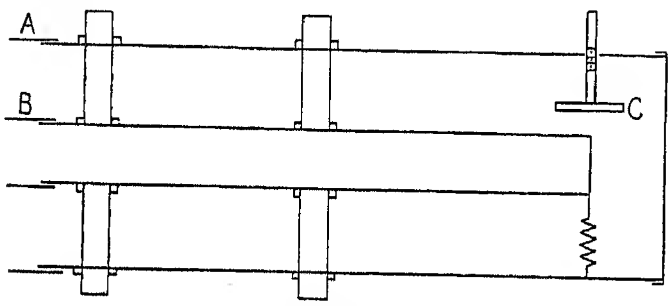


Fig. 25

B was equal to the characteristic impedance of the main feeder. This adjustment was performed by preceding the terminator by various sections of feeder such as $\frac{1}{8}$, $\frac{1}{4}$, $\frac{3}{8}$, $\frac{1}{2}$ wavelength, etc., in succession, and adjusting values until equal input resistances were measured on each section. The choice of these particular lengths of feeder depends on the fact that the input resistance is a function of feeder length if misterminated. The departure from the characteristic impedance for the $\frac{1}{8}$ and $\frac{3}{8}$ wavelengths are equal and of opposite sign and depend mainly on the reactance error in the termination. It is therefore possible to adjust the reactance by resetting the condenser to give an input resistance which is the mean of the previous values. In a similar manner the $\frac{1}{4}$ and $\frac{1}{2}$ wavelengths may be used to determine the adjustment of the terminating resistance. The terminator could be adjusted in this manner at 45 Mc./sec. to an accuracy of 0·2 %.

A series of measurements taken at 43, 45, and 47 Mc./sec. for the four lengths, in order to test the selectivity of the arrangement, are shown in Table 1. The figures indicate that the terminating resistance was ·4 Ω too low but was non-reactive, since the $\frac{1}{4}$-wavelength results are consistently low, and the $\frac{1}{2}$-wavelength results high by this amount. There appears to be an uncertainty of measurement of about 0·2 Ω over this frequency range, but no systematic variation.

The resistors are unfortunately subject to change with temperature and to occasional spasmodic alterations. In the original work at Hayes it was found necessary to provide means to keep the terminator at an approximately constant temperature. Later, low-temperature-coefficient resistors were obtained and the heating control was dispensed with. A constant check was maintained on the terminator during the measurement by measuring the d.c. resistance.

Table 1

Input Resistances R_i in Ohms to Lengths of Feeder Specified when Terminated by "Terminating Resistance"

Frequency	43 Mc./sec.		45 Mc./sec.		47 Mc./sec.	
Length of feeder	R_i	R_i-Z_0	R_i	R_i-Z_0	R_i	R_i-Z_0
$\frac{1}{8}\lambda$	77·6	0	77·8	0·2	77·6	0
$\frac{1}{4}\lambda$	78	0·4	78 0	0·4	78·15	−0·55
$\frac{3}{8}\lambda$	77·6	0	77·6	0	77·9	0·3
$\frac{1}{2}\lambda$	77·3	−0·3	77·1	−0·5	77·3	−0·3

(7) THE VISION SYSTEM AT ALEXANDRA PALACE

(a) General Arrangement

The radiating system finally adopted consisted of two rings of full-wave dipoles with the mast in the centre

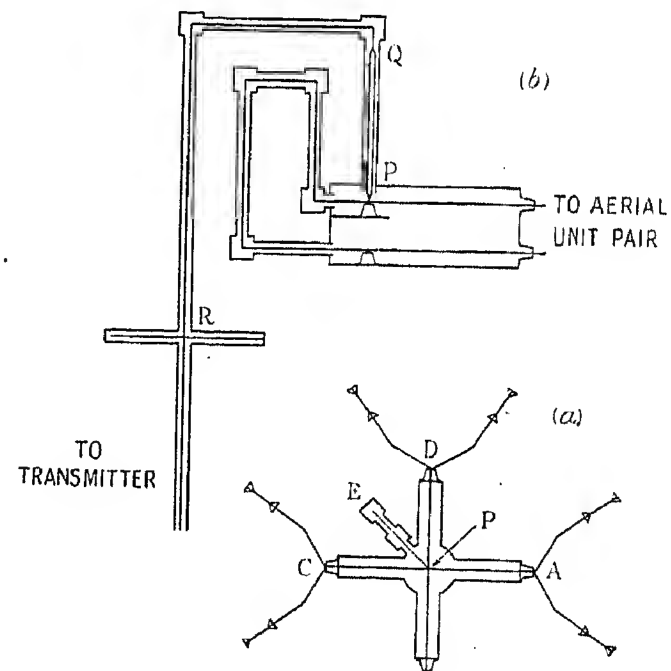


Fig. 26.—Arrangement of aerial and feeder connections.

as illustrated diagrammatically in Fig. 26, in which Fig. 26(a) is a plan view and Fig. 26(b) a false section along the line APE. It is seen that the main feeder is brought to a central point P, from which radiate lines to the various aerial units which are indicated by the

triangles of Fig. 26(*a*). The change from the unbalanced feeder to the balanced aerial system is accomplished by means of a half-wave phase-reversing loop. From the point P (Fig. 26), the point of connection of the feeder to the aerial, a transforming section of approximately $\frac{1}{4}$ wavelength is used to transform the complex aerial impedance to the characteristic impedance of the feeder at the carrier frequency. Beyond this point the main feeder with normal inner conductor runs 450 ft. to the transmitter. A correcting circuit is inserted in parallel with the feeder at approximately 60 ft. from P to reduce the mismatch at the side-band frequencies, and at the transmitter a $\frac{1}{8}$-wavelength transformer is used to transform the characteristic impedance to 50 Ω, which is more suitable for loading the transmitter.

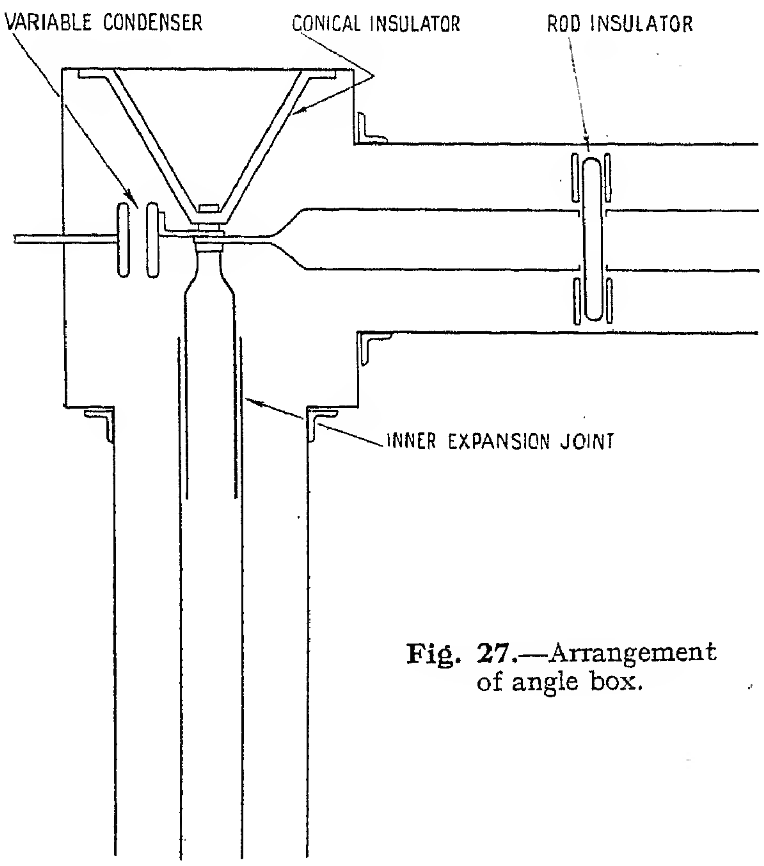


Fig. 27.—Arrangement of angle box.

(b) The Feeder

The main vision feeder runs from the vision aerial platform vertically down to the base of the steel mast, to which it is bonded throughout its length. It passes to the outside of the supporting tower, vertically down to the colonnade, and then about 100 ft. horizontally to a change-over box in the wall of the sound transmitter room. From this box two equal branches lead to the two vision transmitters originally installed in Alexandra Palace. The sound feeder runs beside the vision feeder. Both feeders are laid with as few bends as possible, and those which do occur are right-angle bends in the form of angle boxes.

The feeders consist of concentric copper pipes of 5 in. and $1\frac{3}{8}$ in. diameter, respectively. The characteristic impedance of such a feeder if air-spaced would be 78 Ω and the attenuation 1 neper in 4·8 miles. The inner conductor is located by means of steatite low-capacitance insulators in the form of rods passing through the inner and having sleeves slipped over the ends of the rods of such a length as to centralize the inner (see Fig. 27). The rods are $\frac{3}{8}$ in. diameter and the sleeves $1\frac{3}{4}$ in. long. The insulators were spaced at equal intervals of $\frac{1}{8}$ wavelength at 45 Mc./sec., alternately at right angles, and ferrules were sweated into the holes in the inner to act as guides for the rods and to increase the bearing surfaces. The capacitance introduced by one insulator was 0·4 $\mu\mu$F, and the reduction of characteristic impedance given by equation (14) was 0·35 Ω. The loss was extremely small and was not measured.

A source of trouble with feeders of this type is the condensation of water on the insulators, which can cause losses and introduce impedance irregularities in the feeder. This was encountered in one of the feeders laid at Hayes, but was absent in subsequently laid feeders. Provision was made at Alexandra Palace for drying the feeder, should it prove necessary, by passing a large 50-cycle current through it.

The type of angle box used is shown in Fig. 27. It consists of an approximately cubic brass box containing a large conical insulator and a variable condenser in the form of a disc on the end of a threaded spindle. The same Figure shows an inner expansion joint located at the box.

The feeder was laid from the transmitter end, and at the conclusion of suitable sections, usually at an angle box, the feeder was terminated and the variation of input resistance with frequency measured. Adjustments were made, as in the case of the Hayes vision

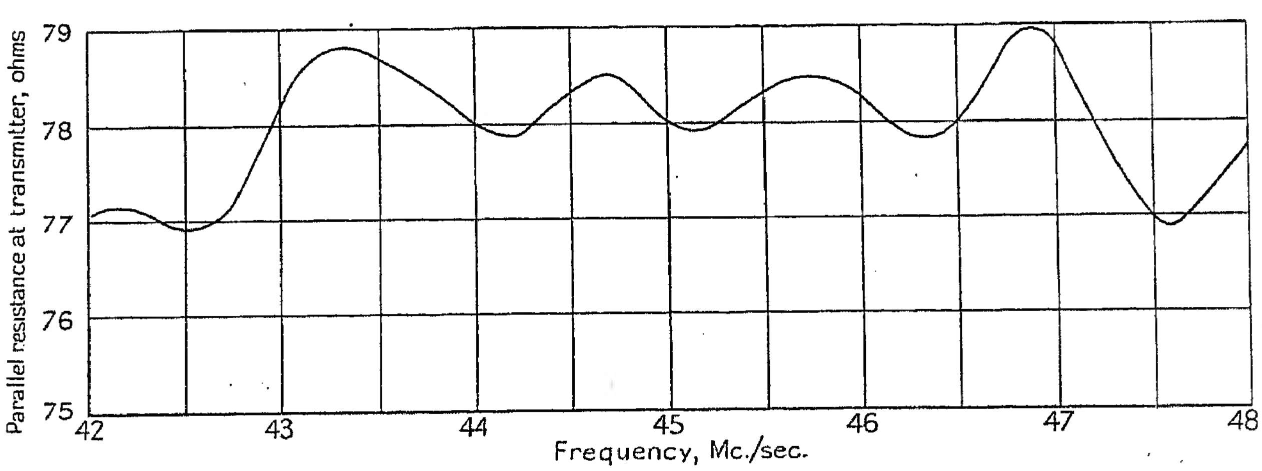


Fig. 28.—Impedance/frequency characteristic of the Alexandra Palace vision feeder, terminated with Z_0 4 ft. below R (Fig. 26).

feeder, to reduce the variation. The final curve of resistance against frequency for the 390 ft. up to the point of insertion of the tuned circuit is shown in Fig. 28. It will be seen that the variation between 43 and 47 Mc./sec. was not greater than $\pm 0 \cdot 5\,\Omega$.

At the transmitter end the arrangement transforming the characteristic impedance to $50\,\Omega$ consisted of a condenser formed by a short length of open-circuited feeder branching from the main feeder about $\frac{1}{8}$ wavelength from the transmitter.

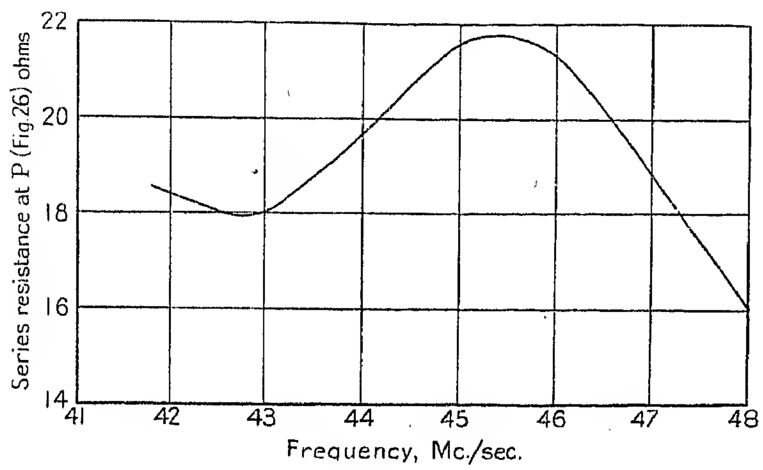


Fig. 29(*a*).—Resistance at P (Fig. 26).

(c) The Aerial Transformer

The design of this transformer is based on the measurements of the impedance between the point P (Fig. 26) and the case. For this purpose a $\frac{1}{4}$-wavelength at 45 Mc./sec. of normal feeder was connected to the point P and the impedance over a range of frequencies was measured by means of the portable impedance gear. The impedances at P at the various frequencies were deduced from these measurements. Figs. 29(*a*) and 29(*b*) show the series resistance and reactance at the point P, plotted against frequency. The curves show a considerable reactive term and a variation of both resistance and reactance with frequency. These variations are not of simple form and are due to the combined effects of the selectivity of the aerial elements themselves and the various small mismatches occurring in the distribution system.

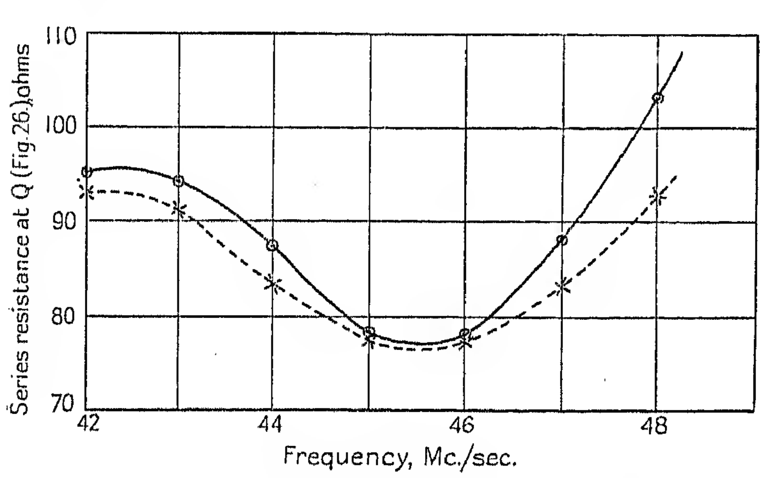


Fig. 30(*a*).—Resistance at Q (Fig. 26).

— O — Values obtained after insertion of transformer section PQ.
- - X - - Values predicted from initial measurements.

In accordance with the principles discussed in Section 5(*a*), a transforming length of feeder with normal outer but $2\frac{9}{16}$ in. diameter inner conductor and 4 ft. 6 in. long was inserted to transform the impedance at 45 Mc./sec. to the $78\,\Omega$ of the feeder. The impedance after inserting this transformer was measured in a similar manner to that at point P. Figs. 30(*a*) and 30(*b*) show the resulting values of resistance and reactance over the same frequency range, together with those predicted from the initial measurements and the constants of the transformer. It is seen that there is substantial agreement between the two sets of values, indicating that the measurements were reliable. The impedance is resistive and matched to the feeder at 45 Mc./sec., but there is a substantial variation at the side-band frequencies.

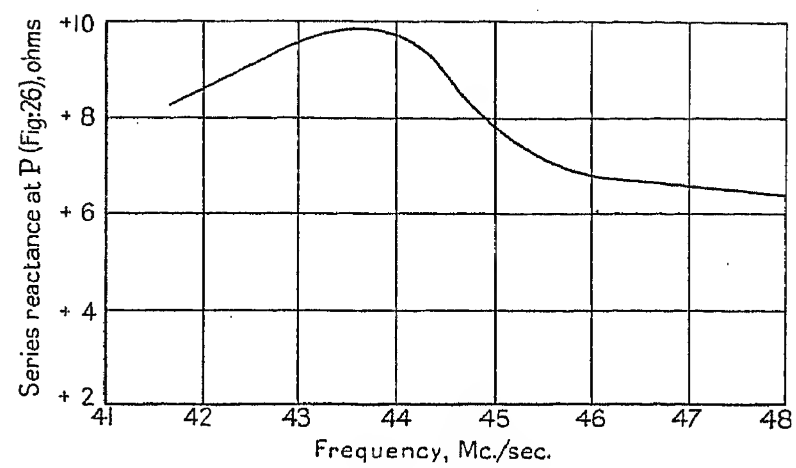


Fig. 29(*b*).—Reactance at P (Fig. 26).

(d) The Correcting Section and Overall Curve

In accordance with the principles discussed in Section 5(*b*), an attempt was made to reduce the effects of this variation by the insertion of a tuned circuit at an appropriate point in the main feeder. From the values given in Fig. 30 it was decided that the best point for the insertion of the circuit was $2\frac{3}{4}$ wavelengths at 45 Mc./sec. beyond point Q. The calculated values of parallel resistance and capacitance at this point are shown in Figs. 31(*a*) and 31(*b*). The resistance is seen to be roughly constant over most of the frequency range, and the capacitance variation is such that it could be

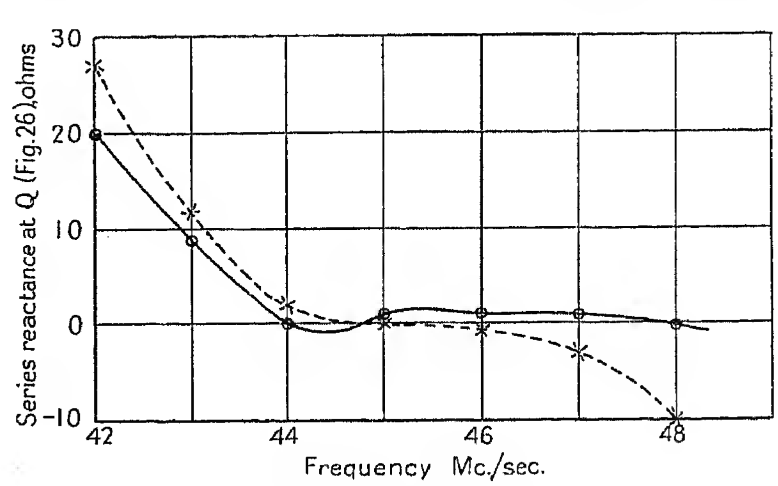


Fig. 30(*b*).—Reactance at Q (Fig. 26).

— O — Values obtained after insertion of transformer section PQ.
- - X - - Values predicted from initial measurements.

substantially reduced by the insertion of a tuned circuit. The feeder was cut in the vicinity of this point, and the impedance was measured at several nearby points. The measured results at the finally selected point of insertion, which was as nearly as could be measured the predicted point, are also shown in Figs. 31(*a*) and 31(*b*). In this case the resistance values agree reasonably, but there is a consistent error of a few micro-microfarads in the reactance measurements.

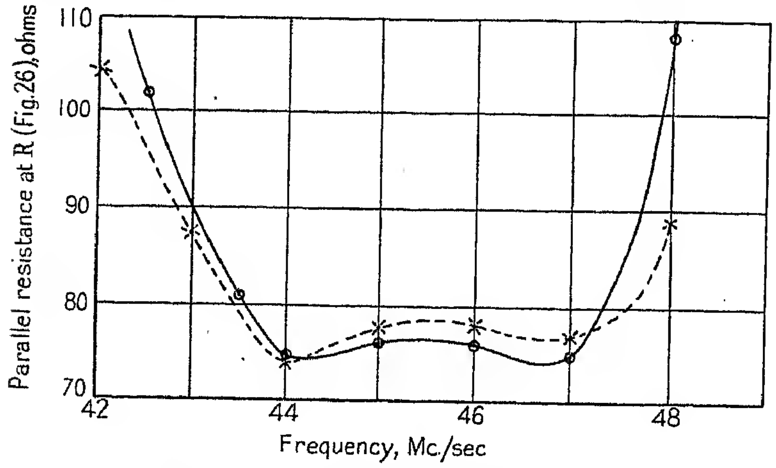


Fig. 31(*a*).—Resistance at R (Fig. 26).
—o— Values measured at point R.
- - X - - Values calculated from measurements of Fig. 30.

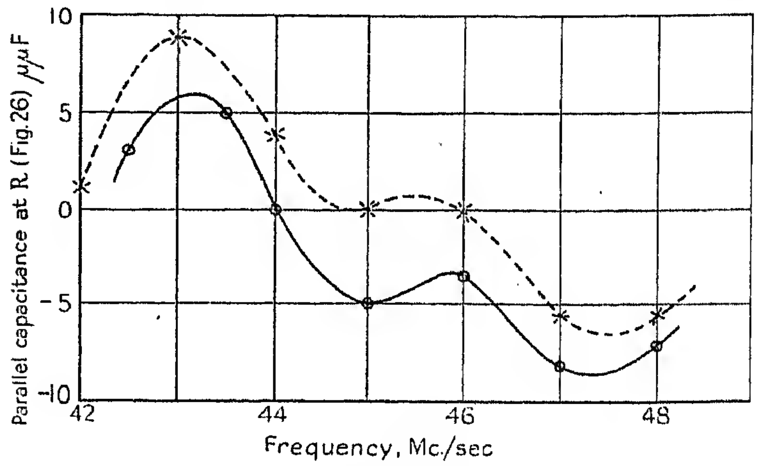


Fig. 31(*b*).—Capacitance at R (Fig. 26).
—o— Values measured at point R.
- - X - - Values calculated from measurements of Fig. 30.

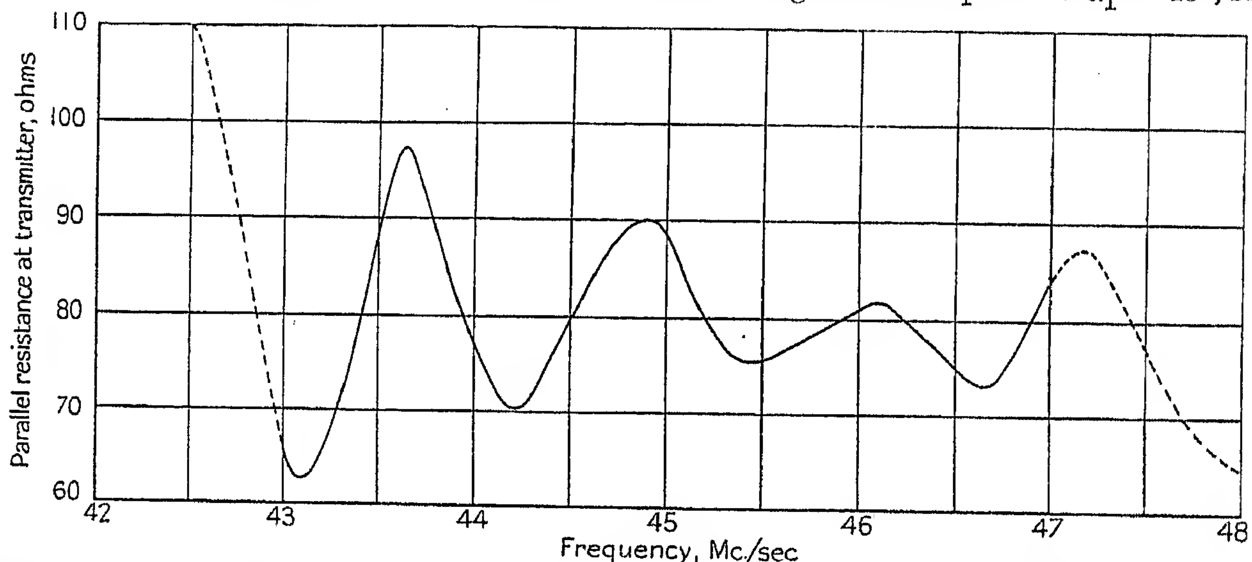


Fig. 32.—Impedance/frequency characteristic of aerial feeder system without parallel $\frac{1}{4}\lambda$ connection.

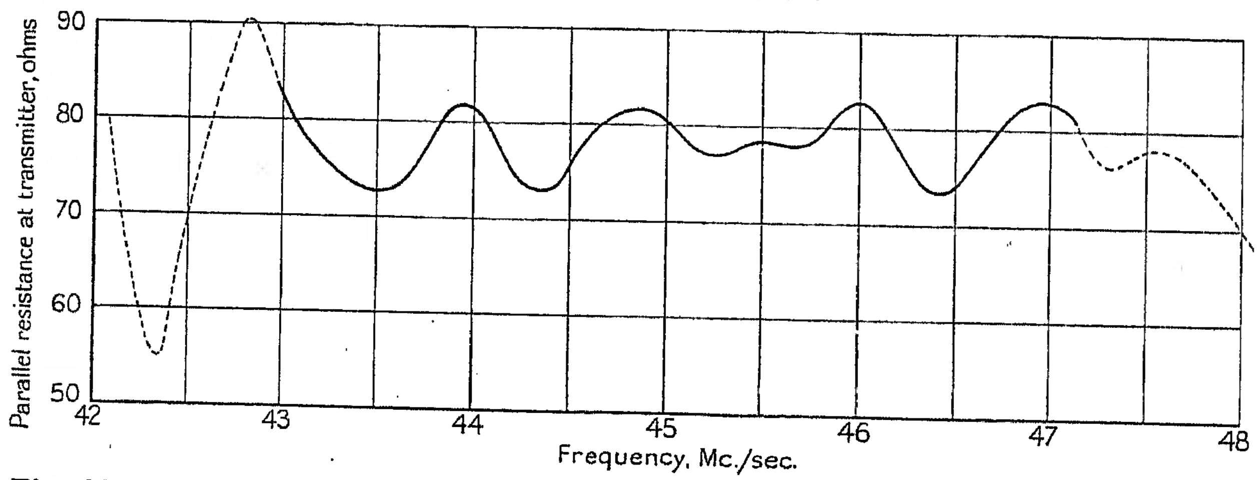


Fig. 33.—Impedance/frequency characteristic of Alexandra Palace vision aerial-feeder system.

Both curves agree, however, in indicating that the tuned circuit to annul the capacitance change should be of such a selectivity to give a capacitance change of 3 $\mu\mu$F per megacycle. The tuned circuit consists of a transverse section of normal feeder $\frac{1}{4}$ wavelength long, one end being open and the other short-circuited. Both ends are capable of sliding extension. With this arrangement both the tuning and the selectivity can be varied in situ. For the selectivity required, equation (21) gives for the length of the open end $\alpha_1 = 43°$, so the open and closed arms were made approximately equal. This form of tuned circuit was suggested by Mr. Rust, of the Marconi Co.

At this stage of the construction the vision feeder was completed from the transmitter to this point, and it was known that it was substantially free from impedance irregularities. It was decided to make the subsequent

measurements from the transmitter end of the feeder, at which point it would only be necessary to measure resistance variation with frequency. Fig. 32 shows the measured variation of input resistance with frequency, the feeder being connected through without the tuned circuit in position.

Fig. 33 shows the same quantity after the tuned circuit had been inserted, adjusted to its optimum setting, and a slight adjustment made to the condenser in one of the upper angle-boxes. It is seen that the overall variation over the frequency range 43 to 47 Mc./sec. is not greater than $\pm$ 5 Ω.

It is possible that this variation might have been further reduced, but considerable difficulty was experienced in making the measurements up the mast under the unfavourable weather conditions prevailing. Further, time was limited by the requirement that the aerial should be complete for preliminary tests on the station. Moreover, the investigations at Hayes indicated that this amount of variation of impedance would lead to no visible distortion.

On completion of the station, observations on the transmitted picture showed no trace of striae due to multiple reflections in the feeder.

(8) ACKNOWLEDGMENTS

These experiments have been carried out in the Research Laboratories of Electric and Musical Industries, Ltd., Hayes, and the authors wish to express their indebtedness to Mr. I. Shoenberg, Director of Research, and to Mr. G. E. Condliffe and the staff of the Laboratories for their encouragement and assistance in the investigation.

In particular, thanks are due to Mr. A. D. Blumlein, who foresaw the effects of a mismatched feeder on the transmitted picture.

This paper deals with the feeder portion of the radiating system at the London Television Station. The aerial portion was designed and installed by the engineers of Marconi's Wireless Telegraph Co., and acknowledgments are due to Mr. Tringham and Mr. O'Neill for their collaboration throughout the whole of the work.

APPENDIX

The Input Impedance of an Approximately Terminated but Irregularly Eccentric Feeder

(a) Formulae for the input impedance as a function of eccentricity.

In this Appendix expressions are derived for the input impedance of a terminated, imperfectly "concentric" feeder in terms of the displacement of the inner conductor from the central position. This displacement is supposed to vary throughout the length of the feeder.

Consider the feeder (see Fig. 34) to be split up into a number of sections each sufficiently short for the eccentricity, and hence the characteristic impedance, to be assumed constant in the section. By an extension of the method of Section 3(*b*), which gave for a single section of the feeder of abnormal characteristic impedance Z_1 the expression

$$Z_i = Z_0 + 2j \sin \gamma_1 (Z_1 - Z_0) e^{-2j\alpha_1} \quad . \quad . \quad (22)$$

it can be shown that for the complete feeder of Fig. 1 the input impedance Z_i is given by:—

$$Z_i = Z_0 + \Sigma 2j \sin \gamma_1 (Z_1 - Z_0) e^{-2j\alpha_1} \quad . \quad . \quad (23)$$

where α_1 and γ_1 are the electrical lengths indicated in the Figure, and the summation includes each section into which the feeder is divided.

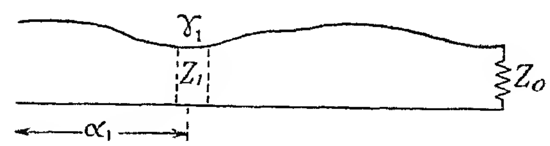


Fig. 34

By dividing the feeder into n equal sections of length γ this formula gives the Fourier series

$$Z_i = Z_0 + \sum_{m=1}^{m=n} 2j \sin \gamma (Z_m - Z_0) e^{-2j(m-\frac{1}{2})\gamma} \quad . \quad (24)$$

or by making the sections infinitesimal so that $\gamma = d\alpha$ the formula gives the Fourier integral

$$Z_i = Z_0 + \int_0^{\alpha_1} 2j(Z - Z_0) e^{-2j\alpha} d\alpha \quad . \quad . \quad (25)$$

where Z is the characteristic impedance at a length α from the input end and α_1 is the total length of the feeder.

The next consideration is to determine the values of the characteristic impedance in terms of the eccentricity of the conductors. Consider two cylinders of radii r_0 and r_1, one surrounding the other and having their axes separated by a distance d, as shown in Fig. 35.

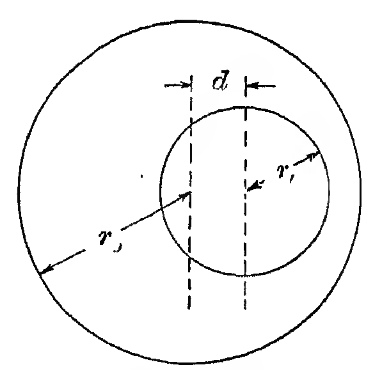


Fig. 35

For this arrangement with air spacing, Moullin* gives

$$C = \frac{0 \cdot 24}{\log_{10} \beta + \sqrt{(\beta^2 - 1)}} \ \mu\mu\mathrm{F/cm.} \quad . \quad . \quad . \quad (26)$$

where

$$\beta = \frac{r_0^2 + r_1^2 - d^2}{2 r_0 r_1} \quad . \quad . \quad . \quad . \quad (27)$$

From this it can be shown that the change in Z_0 due to a small displacement d from the centre is given by

$$\Delta Z_0 = -\frac{60 d^2}{r_0^2 - r_1^2} \quad . \quad . \quad . \quad . \quad (28)$$

* "Radio-Frequency Measurements," 1931, p. 291.

and for a feeder for which

$$2r_0 = 5 \text{ in.}, \quad 2r_1 = 1\tfrac{3}{8} \text{ in.}$$

$$\Delta Z = -10\cdot 4d^2 \qquad (29)$$

where d is in inches and ΔZ in ohms.

(b) Permissible limits of eccentricity.

It is desirable to estimate the tolerances which may be allowed in centring the inner conductor without the introduction of input-impedance variations greater than a given permissible value. The problem does not admit of an exact solution, since if the only information available is that the mechanical constraint prevents a displacement greater than a given value, the actual variation of the displacement, and with it the input impedance, may assume any number of forms. In order to arrive at a useful conclusion two possible configurations of the inner conductor, subject to a given maximum displacement, are assumed.

In the first case the configuration is that giving the greatest possible deviation of the input resistance from the characteristic impedance of the feeder (Z_0) when the conductors are concentric. From the equation

$$r_i = Z_0 + 2\int_0^{\alpha_1}(Z - Z_0)\sin 2\alpha d\alpha \qquad (30)$$

and $\qquad Z - Z_0 = -10\cdot 4d^2$

which applies to this particular type of feeder, it is seen that if d is constrained to be less than d_0 then $(Z - Z_0)$ may vary between 0 and $-10\cdot 4d_0^2$. The integrand of equation (30) is the product of two variables $(Z - Z_0)$ and $\sin 2\alpha$, the former being a function of the configuration. The greatest value of the definite integral will be given by the configuration which gives a maximum value of $(Z - Z_0)$ when $\sin 2\alpha$ is positive, and a minimum value when $\sin 2\alpha$ is negative. In this case, with alternate $\frac{1}{4}$-wavelengths of the two extreme characteristic impedances Z_0 and $(Z_0 - 10\cdot 4d_0^2)$, the value of $(r_i - Z_0)$ is $2 \times 10\cdot 4d_0^2 2x/\lambda$, where x is the length of the feeder. This gives for a permissible resistance deviation of 1 ohm on a feeder of 20 wavelengths the value $d_0 = 0\cdot 035$ in.

The above peculiar configuration of the inner is obviously unlikely to occur in practice, and the tolerance obtained is unnecessarily stringent. A second type of configuration is therefore treated in which the eccentricity within the given limits is assumed to be of a random nature, and a probable value of $(r_i - Z_0)$ is obtained. Serious difficulties arise in specifying the probability of any given configuration, because, owing to the rigidity of the pipe forming the inner, the eccentricity of adjacent sections of the feeder is not independent. In consequence the following treatment is not rigorous, but it is hoped that it would give the right order of magnitude. The feeder is supposed to be divided into n equal sections of length l. This length is chosen such that the eccentricity is approximately constant over one section, but the neighbouring sections are substantially independent. From observations of the degree of bending of the inner pipe these sections were estimated to be of length 2 ft. The final value of $(r_i - Z_0)$ obtained from equation (24) is given by

$$r_i - Z_0 = \sum_{m=1}^{m=n} 2(Z_m - Z_0)\sin\frac{4\pi l}{\lambda}(m - \tfrac{1}{2})\sin\frac{2\pi l}{\lambda} \qquad (31)$$

where Z_m is the characteristic impedance of the mth section. The most probable value P for the sum of this series is the square root of the sum of the squares.

Hence

$$P = \sqrt{\left[\sum_{m=1}^{m=n}\left\{2(Z_m - Z_0)\sin\frac{2\pi l}{\lambda}\right\}^2\right]} \qquad (32)$$

$$= \sqrt{n}\, 2a \sin\frac{2\pi l}{\lambda} \simeq \sqrt{n}\frac{4\pi l a}{\lambda} \qquad (33)$$

where a is the r.m.s. value of $(Z - Z_0)$. If x is the total length of the feeder $n = x/l$ and

$$P = 4\pi\frac{\sqrt{(xl)}}{\lambda}a \qquad (34)$$

The total variation with frequency of r_i is of the order of $\pm P$. Equation (34) indicates that P increases with the feeder length x, as would be expected. The increase of P with the section length l corresponds to the fact that a small number of terms of random phase tend to give a sum greater than a large number of smaller terms which average to more nearly zero. It follows that the non-uniformity of a feeder as measured by P may be reduced by using a more flexible inner conductor, so reducing l, provided that it can be made central within the same maximum limits.

Equation (34) is capable of experimental verification. The steatite rod insulators allow a displacement d parallel to their length of $\frac{1}{32}$ in. and perpendicular to this of $\frac{1}{4}$ in., and the pipe tends to bend appreciably in the 5 ft. 6 in. interval between similarly orientated insulators. Consequently the average displacement may be taken as roughly $0\cdot 15$ in. This gives a value of a of $10\cdot 4 \times 0\cdot 15^2 = 0\cdot 23\ \Omega$. Taking l as 2 ft., then at 45 Mc./sec. equation (34) becomes

$$P = 0\cdot 18\sqrt{x}\ (x \text{ in feet}) \qquad (35)$$

Table 2 gives the mean variation of the input resistance from Z_0 for a number of lengths of this type of feeder which were measured at Hayes, and for the variation of which no other cause than eccentricity is known. Values of P calculated from equation (35) are also included. Since the agreement between the theoretical and measured results is fair, being no worse than the

Table 2

Length of feeder, ft.	79	320	216	85	70	80
Mean deviation from Z_0, Ω ..	0·7	5	2	1	2	0·5
P (calculated), Ω	1·6	3·2	2·6	1·7	1·5	1·6

random variations in the measurements, there is some experimental justification for the theory.

For a permissible value of P of $1\,\Omega$ and $x = 450$ ft. and $l = 2$ ft., equation (34) gives $a = 0 \cdot 06\,\Omega$; the corresponding displacement d from the central position is $0 \cdot 07$ in. For this maximum displacement the greatest possible value of the deviation of the input impedance from Z_0 due to the eccentricity is $5\,\Omega$. It would appear, therefore, that in the design of future feeders of this nature the trouble due to eccentricity would not be serious if the displacement of the inner conductor could be restrained from exceeding about $0 \cdot 05$ in. For greater lengths of feeder the tolerance would be correspondingly restricted.

Recently two papers* have appeared treating certain aspects of the effects of random irregularities in feeders, but they do not directly apply to the case discussed in this Appendix.

* M. Didlaukis and H. Kaden: *Elektrische Nachrichten-Technik*, 1937, vol. 14, p. 13. P. Mertz and K. W. Pfleger: *Bell System Technical Journal*, 1937, vol. 16, p. 541.

[The discussion on this paper will be found on page 475.]

E.M.I. CATHODE-RAY TELEVISION TRANSMISSION TUBES

By J. D. McGEE, M.Sc., Ph.D., and H. G. LUBSZYNSKI, Dr.Ing.*

(*Paper first received* 20*th July, and in revised form* 22*nd October*, 1938; *read before the* WIRELESS SECTION 7*th December*, 1938.)

SUMMARY

This paper gives a short history of the development of two types of cathode-ray tube now used for transmission of television pictures. The construction and mechanism of operation of the "Emitron" is described and its limitations are discussed. A further development, namely the "Super-Emitron," is then described, and its mechanism of operation and its performance are compared with those of the "Emitron." Some other types of transmitting tubes, which have been tested experimentally in the course of this work, are described briefly.

INTRODUCTION

The earliest suggestion that the cathode-ray tube could be used for the transmission of pictures by television was made by Campbell Swinton† in 1908. He realized the limitation of mechanical scanning methods and saw in the then recently-discovered cathode rays a practically inertialess scanning means.‡ He proposed to focus an image of the scene to be transmitted on to a mosaic of photo-electric cells. The elementary cells would become electrically charged due to the liberation of electrons from them by the light, and could be periodically discharged by a beam of electrons which scanned over them. He developed this idea in greater detail in 1911,§ and in 1926|| he attempted to carry it out experimentally, using selenium as a photo-sensitive mosaic in a manner similar to that recently adopted successfully by Miller and Strange.¶ At about this time the number of suggestions for cathode-ray tubes for television picture transmission increases rapidly in the patent literature.**

Apart from cathode-ray scanning the most important development in television transmitting tubes is the introduction of charge storage. This paper is chiefly concerned with transmitting tubes employing this principle.

The essence of the principle is that during part at least of the picture-frame period the photo-electric emission due to the light from each picture point of the image is stored up as a charge on a mosaic of photo-electric cells, each associated with a condenser. The condensers are discharged in sequence by some switching mechanism such as a beam of electrons; the sequence of electrical pulses thus produced constitutes the "picture signal." This principle appears to be implied in Campbell Swinton's suggestions. For example, in his presidential address to the Röntgen Society on the 7th November, 1911,†† he makes the following remarks about the mosaic of photo-electric cells which he proposed to employ in the transmitting tube he suggested: "It is further to be noted that as each of the metallic cubes in the screen acts as an independent photo-electric cell, and is only called upon to act once in a tenth of a second, the arrangement has obvious advantages over other arrangements that have been suggested in which a single photo-electric cell is called upon to produce the many thousands of separate impulses that are required to be transmitted through the line wire per second, a condition which no known form of photo-electric cell will admit of. Again, it may be pointed out that sluggishness on the part of the metallic cubes . . . in acting photo-electrically in no wise interferes with the correct transmission and reproduction of the image, provided all portions of the image are at rest."

Though the storage principle is not mentioned explicitly in this, or any other paragraph, of Campbell Swinton's writings on television, it is difficult to escape the conclusion that it is this principle he had in mind in the first of the paragraphs quoted. This is borne out by the specific and separate reference to the sluggishness of the photo-electric effect in the paragraph immediately following. This interpretation of Campbell Swinton's suggestion is in agreement with that given in 1932 in the excellent handbook on television edited by Prof. Fritz Schröter.†

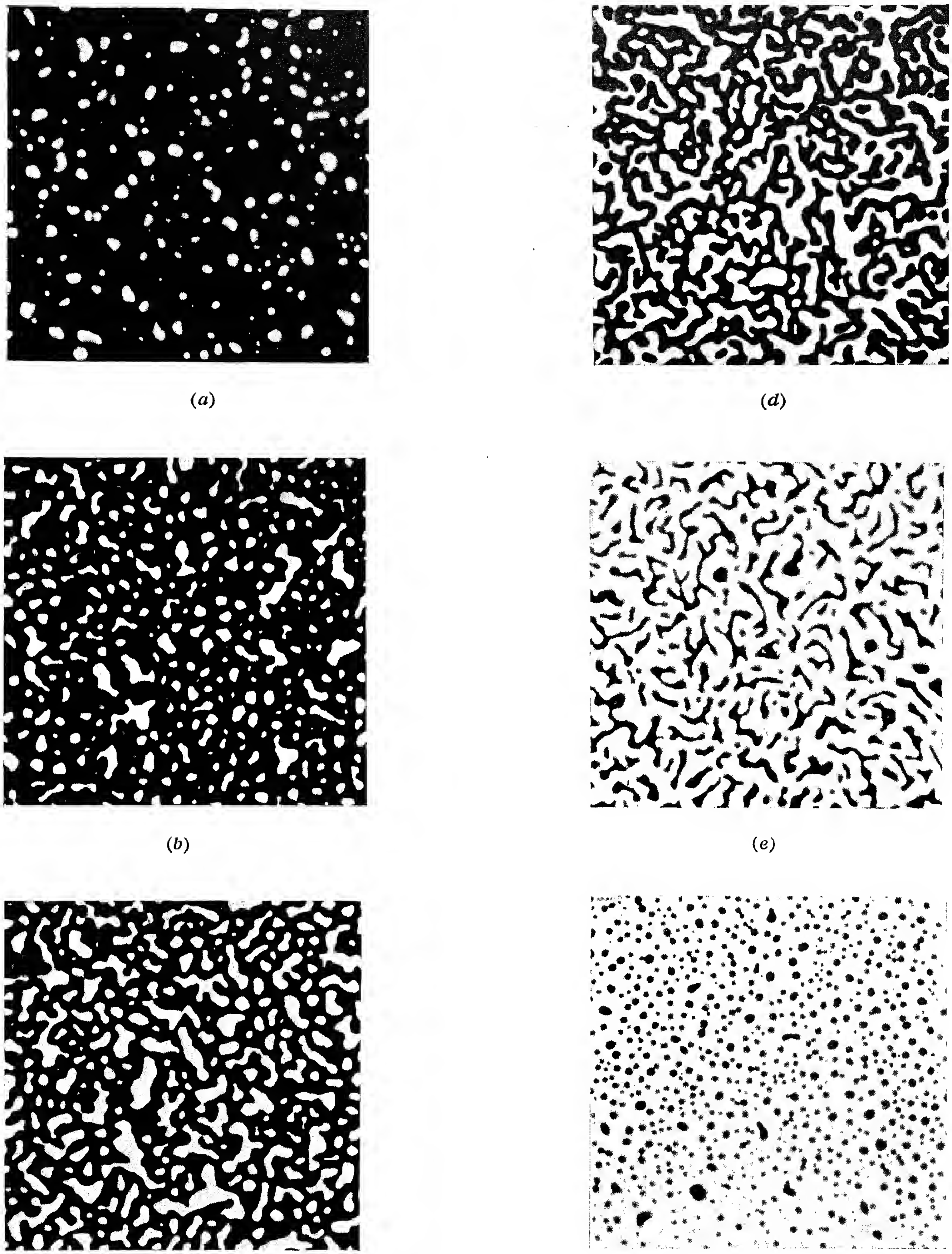
These early schemes were undoubtedly impracticable at the time they were suggested, and it was only after 20 years of technical progress that it became possible to put them into practice. Furthermore, all the schemes proposed seemed to be theoretically unsound‡ and it is probably true to say that transmitting tubes of this kind were in operation experimentally for a considerable time before the essential feature of their mechanism of signal production was understood.

As far as it has been possible to ascertain, the first disclosure in the patent literature of a photo-electric mosaic electrode is due to Zworykin in 1925,§ but it seems significant that though the charge-storage principle is used it is not mentioned in the claims. In fact it appears that this fundamental invention was never patented, although in subsequent publications its importance is emphasized.

The first published description of a successfully operated television transmitting tube employing both cathode-ray scanning and charge storage was due to Zworykin|| in 1933. Though difficult to manufacture and operate, this transmitting tube, named the "Iconoscope" by Zworykin, is a great advance on the earlier mechanical methods. Only 5 % to 10 % of the possible increase in sensitivity due to charge storage is realized in this tube, nevertheless it is sufficiently sensitive to enable high-

* Electric and Musical Industries, Ltd.
† See Reference (1).
‡ For an appreciation of the work of Campbell Swinton on television see an article in *Wireless World*, 1935, vol. 8, p. 591; also *Nature*, 1936, vol. 138, p. 674.
§ See Reference (2).
|| *Ibid.*, (3).
¶ *Ibid.*, (4).
** *Ibid.*, (5).
†† *Ibid.*, (2).

† See Reference (6). ‡ *Ibid.*, (6). § *Ibid.*, (7). || *Ibid.*, (8).



(*a*) (*d*)

(*b*) (*e*)

(*c*) (*f*)

Fig. 3

Plate 2

Fig. 7

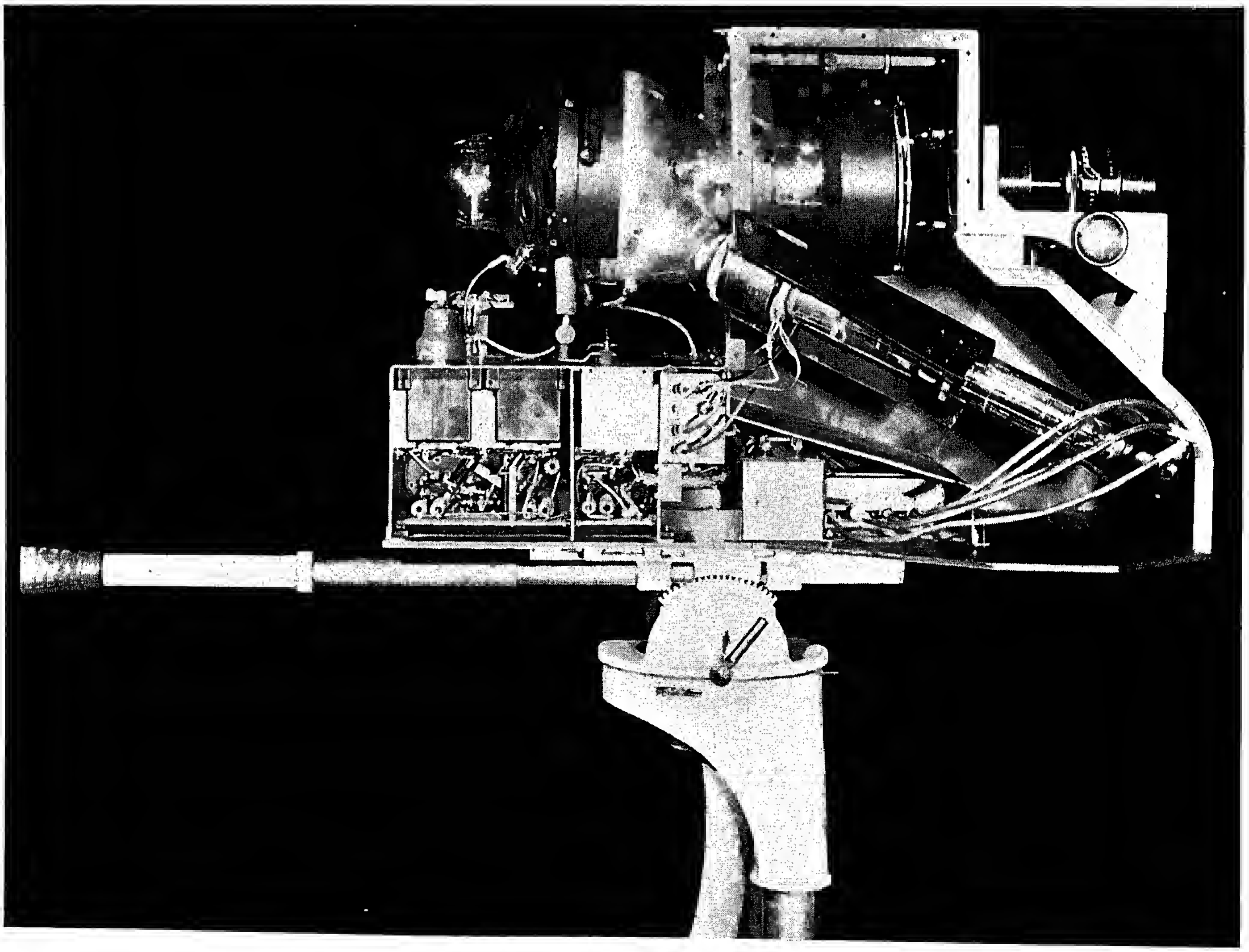


Fig. 8

definition pictures of studio and outdoor scenes with moderate illumination to be televised.

About a year before Zworykin published his description of the Iconoscope, experiments had been begun in the E.M.I. Research Laboratories on the charge-storage type of transmitting tube. These experiments, carried on independently of Zworykin and his co-workers, soon showed that it was possible to generate picture signals in this way. Briefly, the method employed was as follows. An aluminium plate was coated with a thin layer of aluminium oxide by anodic oxidation, and a mosaic of patches of silver was formed on the surface of the oxide by evaporating silver on to the oxide surface through a grid.* This mosaic was mounted as target in a tube, which was then evacuated, and the silver patches were oxidized and activated photo-electrically with caesium. When an optical image was focused on the mosaic while it was being scanned by an electron beam, picture signals corresponding to the optical image were obtained and an image was reproduced on the screen of a cathode-ray tube.

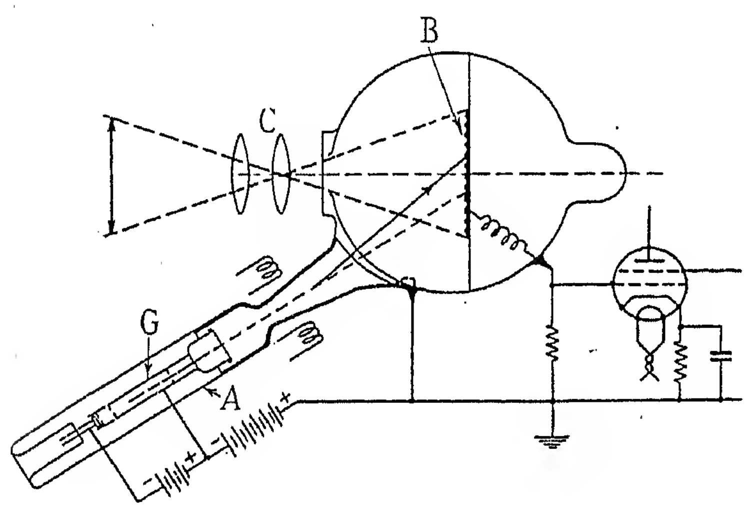


Fig. 1

These experiments led to the development of the Emitron, which is believed to be different in several respects from the Iconoscope and has been in regular service at the London Television Station since its inception in August, 1936. A further development is the "Super-Emitron,"† which was put into service for the first time during the outside broadcast from the Cenotaph in November, 1937. This tube shows a considerable increase in sensitivity and flexibility over the standard Emitron.

THE EMITRON

Construction

The main constructional features of an Emitron are shown in Fig. 1. A spherical bulb of Pyrex glass about $7\frac{1}{2}$ inches in diameter is provided with a neck A in which is fitted an electron gun G which directs a sharply-focused beam of electrons on to the photo-electric mosaic B. A polished flat glass window is sealed on to the bulb as shown, so that an undisturbed optical image of the scene to be transmitted may be focused on the mosaic by means of the lens system C.

* See Reference (9).
† "Super-Emitron" is the trade name which has been adopted for the improved type of transmitting tube described below.

The photo-electric mosaic is formed on the surface of a sheet of mica about 0·001 in. thick and 4 in. × 5 in. superficial dimensions. This sheet of mica is carefully selected for uniformity of thickness and freedom from blemishes of any kind. The reverse surface from that on which the mosaic is deposited is coated with a conducting, highly reflecting metallic layer, generally by painting on several coats of commercial "liquid silver." This sheet of mica is supported on a second circular sheet which fits closely into the bulb. The assembly is flexible and can be rolled into a cylinder while being inserted into the bulb, where it is fixed in position.

Electron Gun Design

A diagram of the electron gun used in the Emitron is shown in Fig. 2. The cathode A is of the indirectly-heated oxide-coated type and is placed immediately behind an aperture in a modulating electrode B. The first anode C extends from the modulator to a short distance within the second anode D, which is formed by depositing a layer of silver on the inside of the neck and part of the bulb, as shown.

Since the dimensions of the mosaic which is scanned by the electron beam are approximately 10 × 12·5 cm., the scanning lines being parallel to the longer sides of the rectangle, it follows that for a 405-line picture the scanning spot must ideally be 0·25 mm. in diameter. Since the scanning beam falls on the mosaic at an angle of 35° to the normal, the actual diameter of the electron pencil on

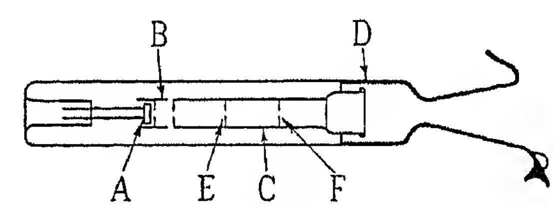


Fig. 2

reaching the mosaic surface must be kept less than about 0·20 mm. Referring again to Fig. 1, it is clear that the distance travelled by an electron from the main electron lens of the electron gun to the mosaic is greatest when the electron beam is at the top of the mosaic and least when it is at the bottom. Since the electron beam is in focus at one distance only from the electron lens it follows that if it is focused at the centre of the mosaic it will be slightly out of focus at both the top and the bottom, and the reproduced picture will lack detail in these regions. This can be corrected by using in the electron gun an auxiliary electrode which is modulated synchronously with the scanning fields in such a way as to change the focal length of the electron lens slightly—sufficiently to keep the beam in focus over the whole mosaic.*

It is found, however, that this can be achieved more simply by taking advantage of the fact that a comparatively small beam current of the order of 0·10 μA is required for scanning the mosaic. It is therefore possible to reduce the "aperture" of the electron lens to a very much smaller diameter than is possible in a cathode-ray tube for television reception, and consequently very much greater depth of focus of the electron beam can be

* See Reference (10).

obtained. Referring to Fig. 2, the aperture E limits the electron beam to a diameter of approximately 2 mm. in the region of the electron lens, which is about 40 mm. in diameter. The aperture F is provided to prevent most of the secondary electrons liberated from the walls of the gun or from the aperture E reaching the mosaic. If such stray secondary electrons reach the mosaic they reduce the efficiency of the tube and produce spurious signals which interfere with the required signals. The fact that a beam of small cross-section suffices has the further advantage that the electron lens need not be carefully corrected, since only a small portion of its cross-section is used.

The Mosaic Mounting

As above described, the mosaic is formed on a sheet of mica which is attached to a second sheet serving to anchor it to the bulb. This construction is not completely rigid and in practice it is often necessary to operate tubes under conditions where they are subjected to considerable vibration; for example, on a movable truck in a studio or in a high wind out of doors. It was soon found that such vibration produced spurious microphonic signals which could be divided into two fairly distinct classes, (1) a low-frequency signal at about 100 cycles per sec. and (2) a higher-frequency signal at about 10^4 cycles per sec. The low-frequency microphonics were found to be due to the vibration of the mica disc supporting the signal plate. Any considerable vibration of this kind would have the effect of throwing the optical image rapidly in and out of focus, and hence would result in a blurred picture.

In operation, the signal plate is held at a positive potential relative to the second anode, and the small changes in its capacitance to its surroundings due to vibration set up voltage fluctuations across the signal resistance. This was eliminated sufficiently for all practical purposes by anchoring the supporting mica disc to the glass walls at four points by soft damping wires.

The high-frequency microphonic signals were eventually traced to a similar effect due to movement between the signal plate and the surface of the mica disc on which it is supported. This movement is partly in the plane of the disc and partly normal to it, and the microphonic signals may be produced in two ways. Firstly, friction produces electrical charges between the signal plate and the mica surface. Slight movements of the signal plate normal to the surface of the supporting mica sheet produce changes of the capacitance between these two surfaces. Thus varying potential differences are set up which lead to spurious signals. Secondly, charges may be produced on the surface of the supporting mica sheet owing to electrical leakage over its surface. Any small movement of the surfaces normal to one another would then produce signals. This type of microphonic signal was effectively eliminated by covering the surface of the supporting mica disc, with which the signal plate is in contact, with a conducting metallic coating. This coating is connected electrically with the signal plate and hence held at the same potential.

Emitrons in which these precautions have been taken can be used under conditions where they are subjected to considerable vibration without microphonic signals being generated.*

The Mosaic Surface

The silver mosaic may be formed on the clean surface of the mica by depositing a continuous sheet of silver and causing this to aggregate by heating in air to a temperature of about 700° C.† If the layer is sufficiently thin it breaks up into a series of discrete areas under surface-tension action, even though the temperature be below that at which bulk silver melts. This phenomenon was first observed by Faraday and was investigated more fully by Beilby.‡ The process of aggregation can be clearly seen in the microphotographs shown in Fig. 3 (see Plate 1, facing page 468);§ these illustrate a series of layers of silver of decreasing thickness each heated to the same temperature. It is seen that when the silver is thick the effect of the heating is to cause a number of holes to appear. As the layer is made thinner these holes increase in size until they merge with one another, leaving islands of silver completely separated. With still thinner layers these islands disintegrate to form still smaller areas.

Exactly the same process is observed in a single silver layer as the temperature of baking is raised from a low value to about 700° C. At comparatively low temperatures (200°–300° C.) the holes appear; as the temperature is raised these increase in size until they merge, leaving the discrete mosaic areas which break up further as the temperature is raised still higher.

That this phenomenon is due to surface tension in the layer may be seen by comparing these photographs with those obtained by Beilby|| for the aggregation of oil films under surface-tension forces.

Although photo-electric mosaics formed in this way are in many ways satisfactory, there are a number of disadvantages such as the difficulty of ensuring that the mosaic, which must be formed before being sealed into the tube, is kept clean and free from spots and other blemishes.

A more satisfactory way of forming the mosaic will be described in a later communication by L. Klatzow.

Mechanism of Signal Production¶

When a photo-electric mosaic of the type described above is scanned by an electron beam of about 1 000 volts velocity, each primary electron liberates several secondary electrons from the surface. The behaviour of an insulated conducting target of mosaic character when bombarded by electrons may be shortly recapitulated. Fig. 4 shows a typical curve of the number of secondary electrons released per primary electron as a function of the velocity of the primary electrons. The curve possesses a maximum, the position of which, for most substances, lies between 300 and 400 electron volts. There are two values of the velocity of the primary electrons for which the secondary-emission coefficient is

* See Reference (11).
† *Ibid.*, (12).
‡ *Ibid.*, (13).
§ The authors are indebted to Dr. L. Klatzow, of the E.M.I. Research Department, for these photographs.
|| See Reference (13).
¶ The mechanism of signal production in the Iconoscope was recently discussed by Zworykin, Morton, and Flory (see *Proceedings of the Institute of Radio Engineers*, 1937, vol. 25, p. 1071).

unity, viz. those represented by the points A and B in Fig. 4. We may call A and B the "first and second cross-over points" respectively. Under the influence of the primary beam and the loss of secondaries the target takes up a certain equilibrium potential. There are three different points on the characteristic for which the potential of the target will be stable.

First, if the primaries hit the target with a velocity below that corresponding to A, they will charge the target negatively, until it stabilizes at the potential of the cathode from which the primary electrons emerge, i.e. at the potential represented by the point O on the curve.

The second possibility occurs when the velocity is between those corresponding to the points A and B. In this case the secondary-emission coefficient is greater than unity, and equilibrium will be attained when, for each primary electron striking the target, one secondary electron (of the number released) reaches the collecting electrode. This equilibrium potential depends mostly upon the geometrical configuration of the tube and the secondary-emission coefficient. The equilibrium potential of the target will be in the neighbourhood of that of the collecting anode—a few volts above or below.

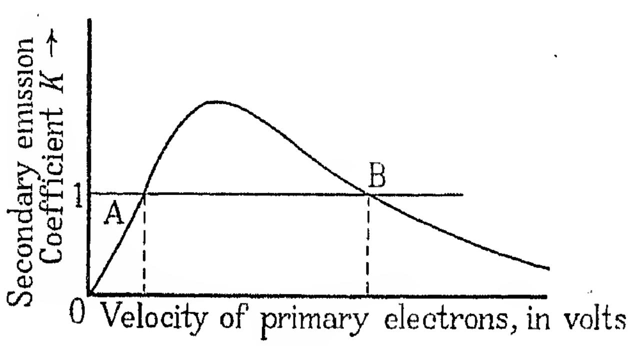


Fig. 4

The third possibility occurs when the primary electrons reach the target with a velocity greater than that corresponding to the second cross-over point B. In this case the number of secondaries released will be less than the number of primaries which strike the target, and the target potential will, therefore, become more negative until the primaries are decelerated to a velocity corresponding to the second cross-over point. When this point is reached one secondary electron will be released per primary and the potential of the target will be stable. In this case the equilibrium potential is independent of the potential of the collecting anode but has a value fixed with respect to that of the cathode. It is thus possible to set up a potential difference between the target and the collecting electrode.*

The problem is complicated when a target consisting of an insulator or of a mosaic of minute conducting elements deposited on an insulator is scanned by a fine pencil of electrons. This is the condition which occurs in the Emitron. The potential across the surface of the target will no longer be uniform but will vary from point to point, and from instant to instant as the scanning beam explores the surface. The average potential of the target at any instant will be determined by the secondary-emission coefficient of the surface and the geometry of the electrodes. For example, at a velocity of the primary electrons for which the ratio of secondaries to primaries is greater than 1, the potential will be such that on an average during at least one scan the number of secondaries arriving at the collecting electrode will be equal to the number of primaries which reach the mosaic. In the Emitron, where the collecting electrode is the second anode of the gun, it has been found that the average potential of the mosaic when scanned in the dark is about 1·5 volt negative with respect to the collecting anode. The value obtained by Zworykin* and his co-workers for the Iconoscope is in agreement with this figure.

On the other hand, the potential of an elementary area on the target of the size of the scanning spot will vary periodically as the beam scans the surface. Each primary electron liberates K secondaries which have a velocity distribution varying from almost zero to the velocity of the primary electrons, but the great majority of these secondaries have velocities of the order of 2 to 4 volts. It is clear that, if the point on the mosaic on which the beam impinges is initially uncharged, it will at first lose practically all the secondary electrons liberated from its surface, and consequently will begin to rise rapidly to a more positive potential. As it rises in potential the slower secondary electrons will find it impossible to escape to the surrounding parts of the mosaic, and finally the element will reach a potential such that only one secondary electron can escape per primary arriving. It is found that in this state the elements immediately under the electron beam are about 4 volts positive with respect to the surrounding parts of the mosaic. This rise in potential of a scanned mosaic element is shown in Fig. 5.

When the scanning beam has passed an element the latter will collect secondary electrons from other elements being scanned, and its potential will soon return to approximately the average potential.

It follows that there is quite a strong electric field between the mosaic element which has reached this positive potential under the scanning beam and the surrounding elements. Hence any photo-electrons liberated by the light from the elements that are about to be scanned will be collected by the elements that are being or have just recently been scanned, and the electric field is sufficient to saturate all such photo-electrons up to a distance of the order of 1 cm. in front of the scanning beam. Thus the scanning beam builds up a positive potential front as it scans over the mosaic, which in turn builds up a distribution of electrostatic charges in front of it, corresponding to the light distribution in the image, thus giving very efficient charge storage over a small fraction of the total frame period.

This phenomenon was called "line sensitivity" by Zworykin. In the authors' opinion the Emitron operates almost entirely on this line sensitivity. The integrating sensitivity, i.e. that part of the signal which is due to storage over the whole of the frame time, is comparatively low. This can be seen from the following observations.

A fast-moving object which moves in a direction parallel to the scanning lines is imaged on the receiving screen as a series of pictures, each of which is perfectly sharp, with no signal between the single pictures. This is due to the mosaic elements becoming substantially

* See Reference (14).

* See Reference (15).

sensitive only immediately before the scanning beam reaches them. If an integrating sensitivity were present to any appreciable degree, the picture of the object would have a sharp leading edge with a blurred trail behind extending to the leading edge of the next picture.

In transmitting pictures from cinematograph film it is found that if the image is projected continuously on to the mosaic while the electron beam is scanning the mosaic, very much smaller light intensity is required than if the separate pictures are projected on to the mosaic during the intervals between frames when the electron beam is suppressed. Even when allowance is made for the difference in the time that the mosaic is exposed to the light image, the efficiency of the tube appears to be of the order of 10 times greater in the former case.

Finally, the result of applying a positive potential to the collecting electrode leads to the same conclusion. It can be seen by reference to Fig. 5 that the actual picture signal is derived from the differences in the potential changes which occur when a dark and when an illu-

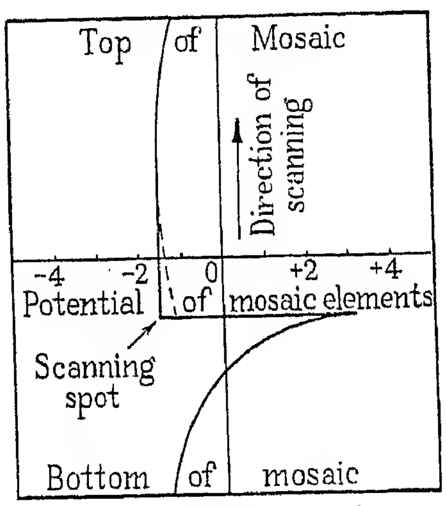


Fig. 5

minated element is scanned by the beam. If the potential of the second anode, which acts as the collecting electrode in the Emitron, is suddenly raised to a positive potential with respect to the signal plate which is sufficient to ensure saturation of photo-electrons liberated from the mosaic, it is found that a strong signal is obtained, which slowly dies away. Now a signal can only be obtained if the stream of secondary electrons leaving the mosaic varies according to the distribution of light in the optical image. Hence, the secondary electron emission cannot be saturated, since then no signal would be produced, although the potential between the second anode and mosaic is sufficient to saturate slow electrons liberated from the mosaic in the absence of the scanning beam. It follows that the flow of secondary electrons from the mosaic to the second anode must be controlled by the potentials of the mosaic elements in the immediate neighbourhood of the scanning spot, rather than by the potential of the second anode, since a potential difference of 3 or 4 volts between the element which has just been scanned and the element which is just about to be scanned requires a field of the order of 100 volts/cm., while the field due to the saturating potential on the second anode is only of the order of 10 volts/cm. at the mosaic surface.

The application of the saturating potential will, however, result in almost all photo-electrons which are liberated by light at points of the mosaic some distance from the scanning spot being collected by the second anode. Hence, a stronger electrostatic image will be built up than is obtained in the normal working of the tube, and the signals will be proportionately stronger. Because of the saturating field the proportion of secondary electrons that escape from the mosaic will be slightly increased, and the average potential of the mosaic will drift towards the potential of the second anode. Eventually it will reach a new equilibrium potential of a few volts negative with respect to the second anode, and the signals will return to normal. The time taken for the mosaic to reach its new equilibrium potential depends on the scanning beam current and the capacitance of the mosaic elements to the signal plate.

If the potential of the collecting anode could be raised to a sufficiently high value to saturate all the secondary as well as photo-electrons, the number of electrons leaving the mosaic would be independent of the illumination of the scanned element, and consequently no picture signal would be produced. It has not been possible to apply a sufficiently high potential between the second anode and signal plate to realize this state completely, but an applied potential of 750 volts was found to produce initially a weak signal, which increased considerably as the mosaic potential drifted towards second anode potential, passed through a maximum, and then returned to normal.

THE SUPER-EMITRON

Theory of Operation

As described above, the Emitron operates with a low efficiency, of the order of 5 % of the theoretical maximum. The reason for this low efficiency is the lack of saturation of the photo-emission from the mosaic during most of the frame period. Another reason, though probably not so important, is the spread of secondary electrons released by the scanning beam from the mosaic. These secondaries neutralize the charges stored on the mosaic elements and also generate spurious signals, as, for instance, a low-frequency component known as "tilt" which is superimposed on the picture signals. The tilt may be corrected by suitable electrical circuits.*

It was considered† that by separating the two functions of photo-emission and charge storage it might be possible to improve upon the efficiency of the Emitron.

A tube which enables this suggestion to be carried into practice is shown diagrammatically in Fig. 6. The optical picture to be transmitted is focused on a continuous transparent photo-surface P. An electron beam is generated at this surface which varies in density across its cross-section according to the illumination. This electron beam is accelerated by the electrode A_2 and focused electron-optically by means of the magnetic lens L on to the storing mosaic M, which is backed by a signal plate S. The mosaic is not photo-sensitive. The beam of photo-electrons projected on to the mosaic generates there a charge distribution which corresponds to the optical picture on the photo-cathode P. The mosaic is scanned in the usual manner by an electron

* See Reference (16).

† *Ibid.*, (17).

beam from a gun G, thus restoring the elements to an equilibrium potential.

There are three striking advantages in the new tube; (1) the improvement in the efficiency of the photo-cathode, (2) the multiplication of the charges on the mosaic by secondary emission, (3) and the improved optical conditions.

In the normal Emitron the photo-sensitivity is limited to about 12 μA/lumen. Continuous photo-cathodes can at present be made with photo-electric sensitivities more than twice that obtained from mosaics in the Emitrons. There are several reasons for this. For one thing, the ratio of the surface covered by photo-electric material in a mosaic to the total area is only of the order of 50 %. Furthermore, the Emitron mosaic has to carry out two functions—it has to emit photo-electrons and to store charges. For storing charges efficiently the insulation between the mosaic elements must be high, and the quantity of caesium which is applied in forming the photo-mosaic is therefore limited as a compromise between sensitivity and insulation.

The other main feature of the new tube is the secondary amplification at the mosaic surface. Under the impact of each photo-electron from the cathode P a number of secondary electrons are liberated at the mosaic and the elements are left with a net positive charge. The sign of the charge stored on the mosaic is the same as in the Emitron, and the signals therefore have the same sense. The amount of charge stored is multiplied according to the secondary emission-coefficient K, the multiplication factor being $(K - 1)$. For caesiated surfaces the factor K is of the order of 7 to 9 for an optimum primary velocity of the electrons of about 400 volts. Therefore, under the same condition and for equal photo-sensitivities it is to be expected that the signal from the new tube should be 6 to 8 times stronger than that from the standard tube.

Comparative measurements have been made on the signal output of a number of Emitrons and Super-Emitrons for equal conditions of illumination and beam current. These measurements showed that for equal photo-sensitivities the new tube gave about 10 to 15 times the signal amplitude of the Emitron. In a few cases the ratio was considerably higher.

This factor, therefore, is too large to be explained in terms of secondary-emission amplification only, but is satisfactorily explained if account is taken of the different velocities of the photo-electric and secondary electrons. As was shown above, the mosaic elements are brought to an equilibrium potential by the scanning beam, which potential is very close to that of the second anode of the gun. The integrating efficiency of the Emitron is small compared with the "line sensitivity," since the photo-electrons can only escape from the elements a short time before the scanning beam strikes them. The photo-electrons released by visible light from the photo-sensitive mosaic have velocities corresponding to approximately 1 electron volt. In the new tube, secondary electrons are released from the mosaic elements. The bulk of these have velocities of several electron volts, and a certain proportion have much higher velocities. They will, therefore, escape more readily under the same field conditions and reach the second anode. This will result in an increase of the integrating efficiency, since a substantial number of electrons will be able to leave the elements between scans. This is confirmed by the observation that fast-moving objects imaged by the new tube leave a blurred trail behind them in the picture, the leading edge being sharp.

Construction

A diagram and a photograph of this type of tube as now in production are shown in Fig. 6 and in Fig. 7 (see Plate 2) respectively.

The electron gun and the mosaic are arranged in the same relative positions as in the Emitron. The chief difference is the addition of the projection neck for the electron optical conversion of the light picture into the electron picture. At the end of this neck is an optically flat window in front of which the photo-cathode is fixed. It is of the transparent type formed on a mica disc approximately 60 mm. in diameter. The cathode is surrounded by a short cylinder for concentrating the photo-electron

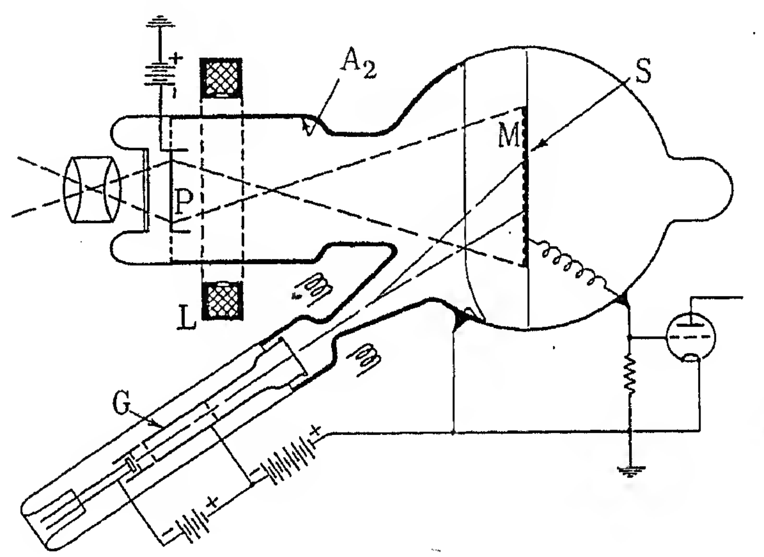


Fig. 6

beam. The second anode of the scanning gun is extended into the projection neck and serves to accelerate the photo-electrons. It is formed by depositing a silver coating on the walls of the tube. The beam of electrons from the photo-cathode is focused on the mosaic by a magnetic lens L of the ironclad type. The coil, however, also produces a rotation of the picture about its axis, and, in order to correct for this rotation, the tube is rotated in the camera through the same angle, and in the same sense. A photograph of the tube in the camera is shown in Fig. 8 (see Plate 2); the focusing coil and the rotation of the tube in the camera are clearly visible. The electron optical magnification between mosaic and photo-cell is approximately 4 : 1. The mosaic consists either of an aggregated aluminium layer on mica or of plain mica which is coated on the back with the usual conductive coating to form the signal plate.

As the size of the optical picture on the photo-cathode is much smaller than that on the mosaic of the Emitron, lenses with shorter focal lengths are used. Furthermore, owing to the increased sensitivity of the new tube, smaller lens apertures can be used for the same illumination

resulting in greatly increased focal depth of the transmitted pictures.

Careful screening in the camera is necessary to prevent interference between scanning and focusing magnetic fields.

When used in this way the new type of tube gives very satisfactory results and has been frequently used by the B.B.C. when the conditions are such that very little light is available or where telephoto lenses are necessary.

The sensitivity of the new type of tube can be still further increased if the electron beam forming the picture is amplified by secondary emission one or more times before reaching the mosaic.* Development along these lines is being carried out and shows considerable promise.

OTHER FORMS OF CATHODE-RAY TUBE

A number of other forms of television tubes which may be mentioned here have been constructed and used experimentally.

The type of tube described above may be used under different operating conditions from those mentioned.† Referring to Fig. 4, if the photo-electrons are accelerated sufficiently to strike the mosaic with a velocity much beyond that corresponding to the second cross-over point B, they will release only a few secondaries and will charge the elements negative. The scanning beam strikes the mosaic with a velocity corresponding to a point between A and B and restores them to an equilibrium potential. A tube operated in this way produced signals the sense of which was opposite to those produced by the standard tube. This method, however, does not give secondary amplification.

Another method is to scan the mosaic with a beam the velocity of which is greater than that corresponding to the second cross-over point.‡ A potential difference is then established between the mosaic and the second anode, which is equal to the difference between the potential of the latter and that of the second cross-over point. If this difference be made large enough, the whole of the emission from the mosaic will be saturated and collected on the second anode. There will be no spread of secondary electrons and therefore no tilt. Furthermore, there will be the full theoretical efficiency due to the complete saturation of all the electrons from the mosaic. The photo-electrons are projected on to the mosaic with a velocity corresponding to the peak of the secondary-emission characteristic in Fig. 4. Those elements which are hit by photo-electrons in the intervals between scans rise in potential and the scanning beam restores them to their equilibrium potential at the second cross-over point. The sense of the signal is the same as that of the standard tube.

As the scanning beam releases very nearly 1 secondary electron per primary, its impedance is very high and consequently large scanning-beam currents must be used in order to restore the elements completely to their equilibrium potential during a single scan. It can easily be shown that an element is restored by the beam to its equilibrium potential exponentially with a time-constant $T = C/(i_p a)$, where C is the element-to-signal plate capacitance, i_p is the primary current in the scanning beam, and a is the slope of the secondary-emission characteristic at the second cross-over point. To ensure complete discharge the time-constant must not exceed about one-third of the time the beam remains on the element. It is necessary, therefore, in order to keep the beam current small, to reduce the element to signal-plate capacitance and to choose a material for the mosaic for which the slope of the secondary-emission characteristic at the second cross-over is as large as possible.

Carbon was found to be the most suitable material for this purpose, and a tube with a carbon mosaic was successfully worked in this way. A very much increased sensitivity and a complete absence of "tilt" were obtained with scanning-beam currents of 55 μA, whereas normally the beam current must be limited to about 0·1 μA to reduce "tilt" to manageable dimensions.

Other forms of tube have been constructed and used in which the necessity for scanning the mosaic at an angle is avoided, and it is therefore unnecessary to supply circuits to correct for trapezium distortion. In the normal Emitron this was done by making the signal plate transparent* and projecting the image on to the mosaic through the signal plate. The sensitivity of such a tube is, however, comparatively low.

Finally, trapezium distortion may also be avoided by using a "double sided" mosaic. In these mosaics the elements are in the form of insulated rivets in a mesh grid which acts as signal plate.† The rivets project on both sides of the grid and may be photo-sensitized on one side. The image is focused on this side and the scanning beam explores the reverse side, the axis of the gun being normal to the plane of the grid. Such double-sided mosaics may also be used in the type of tube where the electron image is focused electron-optically upon the mosaic.‡ In this case it is not necessary to photo-sensitize the rivets.

Acknowledgments

Acknowledgments are due to Mr. I. Shoenberg, the Director of Research; to Mr. G. E. Condliffe; and to the staff of the Research Department, among whom Messrs. L. Klatzow, J. E. I. Cairns, and H. Miller, should be mentioned.

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(17) LUBSZYNSKI and RODDA: British Patent No. 442666, 12/5/1934. At about this time, work on the same lines was commenced in America (see IAMS and ROSE: *Proceedings of the Institute of Radio Engineers*, 1937, vol. 25, p. 1067).

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DISCUSSION BEFORE THE WIRELESS SECTION, 7TH DECEMBER, 1938, ON THE PAPERS BY MESSRS. CORK AND PAWSEY (SEE PAGE 448) AND MESSRS. McGEE AND LUBSZYNSKI (SEE PAGE 468)

Mr. L. H. Bedford: With regard to the paper by Messrs. Cork and Pawsey, it is a matter for some surprise that so high a degree of adjustment and refinement in regard to feeders and terminations is either necessary or possible. It would seem, however, that the accuracy of the results given may be somewhat artificial. On the one hand, we are working in zones where the impedance of 1 in. of wire is very high relatively to the impedance changes concerned; on the other hand, it may be questioned whether the representation of a feeder as a distributed inductance and capacitance can be pushed this far, bearing in mind that this representation is after all only an approximate form of a complicated field theory. The results, however, appear to be adequately justified in practice, not only as shown by the authors' curves but also as seen by any viewer whose receiver does not itself introduce more frequency distortion than that with which the authors are concerned.

The paper by Messrs. McGee and Lubszynski considerably clarifies the relation between the Emitron and the Iconoscope. It would appear from the data given that the distinction is perhaps more one of history than of function.

The great advance which has been made in transmission in the last 2 years is largely attributable to improvement in the Emitron camera, but there is still a shortcoming in connection with film transmission. In my opinion the use of a Mechau projector is not an entirely elegant solution to the problem, and in practice there are evidently great difficulties of adjustment, as is evidenced by the frequent occurrence of flicker or differential illumination of the interlaced rasters. The original method employing intermittent film movement and making use of the "memory" of the Emitron was theoretically more elegant, and failed only because of the imperfect memory characteristics of the Emitron. The paper clarifies this phenomenon and would indicate that with developments along the lines of the Super-Emitron a reversion to the original film method might be contemplated.

Mr. D. C. Birkinshaw: At the time of installation of the feeder Messrs. Cork and Pawsey were apprehensive of the effect of weather upon it; they thought that damp conditions might lower the feeder resistance, and possibly introduce capacitance variation. Measurements made over the last 2 years, however, show that the resistance of the feeder is more than 10 megohms in dry weather, and never descends below about 30 000 ohms in wet conditions. There is also no evidence that weather conditions introduce any capacitance variations. Although means have been provided for electrically drying out the feeder if necessary, it has so far been unnecessary to bring them into operation.

One interesting feature of the feeder which is not mentioned in the paper is the fact that it was unfortunately impossible in one case to obtain a pipe of the correct diameter for the inner conductor in connection with the first transformation to 78·5 ohms, and it was necessary, therefore, to transform to an impedance somewhat greater than this figure, and insert a further transforming section to correct for the error.

Turning to the paper by Messrs. McGee and Lubszynski, I should like to mention that in the early days of the Emitron its colour response was characterized by an emphasis of the red and infra-red end of the spectrum. This introduced certain make-up problems, and gave the picture a peculiar appearance similar to that of a photograph taken on an infra-red-sensitive plate. This was undesirable, and was later overcome, and the tubes now have their peak response in the green region. This considerably improves the quality of the picture, and lessens the difficulty of the make-up problem. I should like to ask the authors how this change in colour response was effected.

Mr. D. C. Espley: Messrs. Cork and Pawsey state that the feeders work from a transmitter termination of 50 ohms to an aerial impedance of about 20 ohms. Is it not possible to use a feeder of lower characteristic impedance? It seems to me that the only limitation would be the voltage on the feeder (about 1 100 volts at present). The change could be made by increasing the diameter of the inner conductor, or preferably by reducing the outer, and in this connection it would be interesting to know the significance of the radius ratio of 3·6. I cannot see why it should be so important to keep to the optimum

attenuation figure resulting from this ratio, because the loss in a 450-ft. feeder of that kind works out at less than 0·2 db.

I should be interested to know whether in the course of these precise impedance measurements any effects due to the proximity of the resonant systems of the sound aerial were noticed. In experiments in this general field we have had trouble with sliding joints, as they can introduce appreciable series resistance. Have the authors also noticed such effects?

It would be interesting to have details of the steady-state propagation characteristics of the transmitter, expressed as the relation of the current in the output stage to the current in the aerial. Further, would the order of accuracy demanded of the authors' impedance measurements show up transmission irregularities equally well?

Turning to the paper by Messrs. McGee and Lubszynski, was the lens correction for focus on the mosaic applied along one dimension of the picture, or was it a two-dimensional correction? I notice that the camera is tilted through a certain angle in order to compensate for the image rotation mentioned by the authors, but would not it be possible to get over that effect equally well, and perhaps in a more convenient manner mechanically, by rotation of the deflection field? Such rotation would not have any effect on the depth of focus, which is already taken care of by the use of a very small beam current.

There is one last point to which I should like to refer. In observing received pictures it occasionally seems that the camera has a spot of low sensitivity in the middle of the field. I suppose that this could be due to one of two causes—either to a simple electron burn, or to negative-ion poisoning. Is any device provided for obviating either of these troubles?

Mr. A. H. Mumford: I should like to know what factor determined the size of feeder adopted by Messrs. Cork and Pawsey. Was it the permissible eccentricity of the inner conductor? The principal causes of reflections appear to be impedance irregularities at connecting boxes, and it is stated that these were compensated for by means of variable capacitors. It is not quite clear whether it was necessary to introduce other variable capacitors in the straight portion of the line to compensate the effects of varying eccentricity of the inner conductor. I notice that the maximum eccentricity which the authors suggest should be allowed is 0·05 in., whereas for the Alexandra Palace feeders the figure was 0·15 in. Have they any scheme in mind for reducing the magnitude of this variation in any future stations which they may be called upon to put up?

It would have been interesting if the paper had given some more information as to the transmitter and the aerial circuits. I notice that although the authors find an impedance match with artificial loads of $\pm$ 0·5 ohm between 43 and 47 Mc./sec., as soon as they introduce the aerial system the figure goes up to $\pm$ 5 ohms. This suggests that possibly the next advance should be in the direction of producing an aerial which has a lower impedance variation. Can the authors say whether the limits to which the aerial and transmission line were adjusted initially have been maintained after some months of operation? It would be interesting to know whether frequent checks of this are made by the maintenance staff, and, if so, the extent of the variations observed.

Turning now to the paper by Messrs. McGee and Lubszynski, considerable attention is paid to the sensitivity of the television-transmission tubes, but obviously the signal/noise ratio obtained from the tubes is also of considerable importance and more information on this point would be very valuable. In particular, is the signal/noise ratio of the Emitron better than that of the Super-Emitron or vice versa? Is any improvement in signal/noise ratio considered possible? On page 474 it is pointed out that the new type of tube gives very satisfactory results and has frequently been used by the B.B.C. If it is so outstandingly good, why has it not been used continuously? I should have considered that its increased sensitivity would have been as valuable for studio transmissions as for outdoor work. Finally, it would be interesting if the authors could give some idea of the life and cost of the tubes described in their paper.

Dr. D. Gabor: I cannot quite accept the claim that Campbell Swinton was thinking of charge storage when he wrote the passage quoted by Messrs. McGee and Lubszynski. There is no doubt that he had only photoconductivity in mind, otherwise he would not have referred to the sluggishness of the cells. This effect is eliminated by the principle of using a great number of cells and scanning them in succession. But as regards storage, a photo-conducting cell is a rather different matter from a photo-electric layer backed by an insulator. The photocell integrates over any length of time. On the other hand, the best a photo-conducting cell can do is to reach an equilibrium with the illumination, and this equilibrium is only delayed by the sluggishness due to transient polarization. Miller and Strange have found certain charge-storage effects, similar to those exhibited by every imperfect dielectric, but these are not due to the illumination but to charges produced by the scanning beam. These produce a "memory" effect, which is, however, pronounced not in the bright but in the dark parts of the picture. The illumination itself produces no surface charges.

May I ask Messrs. McGee and Lubszynski whether in the Super-Emitron any measures have been taken or found necessary to eliminate the pincushion distortion which arises in image convertors with electrostatic lenses, or whether the magnetic lens used was sufficiently free from this defect.

At the end of the paper a very interesting tube is described which operates at the second unity point of the secondary-emission curve. It is stated that carbon is a suitable material for the mosaic, as the curve for carbon has a steep slope near B (Fig. 4). It appears, however, from the experiments of Knoll and his collaborators, and I have also found in my own experiments, that the secondary emission of graphite never reaches unity. Was the carbon in question specially prepared?

Dr. R. F. J. Jarvis: The analysis which Messrs. Cork and Pawsey have made in Section (2) of their paper illustrates the primary importance of time response rather than frequency response in determining the behaviour of a television circuit. It will be noted that the procedure which they adopted was to consider the response

of the line and aerial circuit to a sudden change of carrier input voltage, and that by laying down limitations to the form of this response they estimated to a first approximation the greatest unobjectionable impedance mismatch in the frequency range 43–47 Mc./sec.

It is interesting to recall that in the early days of electrical communications, when telegraph signals were the only type of signal transmitted, scientists considered the time response of the circuit in analysing its behaviour mathematically. The Kelvin arrival curve for the submarine cable is a good example of this. With the coming of telephony and music transmission the frequency response of a circuit became more important than the time response, for the following reasons: (*a*) Most speech and musical sounds are quasi-periodic in nature. (*b*) The most important attribute of the ear is its ability to discriminate between sounds of different frequencies. It has been suggested in recent years that the ear is enabled to do this by means of the basilar membrane of the cochlea, with its fibres of different lengths stretched radially like a harp. (*c*) It has been found much more convenient to design complicated circuits on the basis of frequency response rather than time response.

It was also found that the ear was unable to differentiate between the phase of two waves of sound of the same frequency, so that the phase response of a sound transmission circuit was not very important, except possibly in the reproduction of transient sounds.

When we come to television, however, this state of affairs is completely changed. The most important attribute of the eye is its ability to discriminate between the luminous flux density of the light arriving at the eye at different angles to the optical axis. The eye is enabled to do this by the 5 million rods and cones on the retina.

The spot of light forming a television picture moves with uniform velocity, so that its position is a linear function of time. The eye cannot follow the spot, but by reason of the persistence of vision sees at any instant the brightness of the spot at all its positions on the picture over the preceding period of about $\frac{1}{25}$ sec. In this way the variation of the brightness of the spot with time is translated into a variation of brightness in two dimensions in space, thus forming a picture. Thus, although the eye does not see brightness so much as a function of time as of space, the latter function is directly related to time in a picture formed by constant-velocity scanning. The time response of the circuit, which determines the variation of the brightness of the scanning spot with time, is thus the most fundamental in a television circuit.

It is well known that given the time response of a circuit its frequency response can be determined by Fourier analysis, it being remembered, of course, that frequency response is a complex function of frequency. The frequency response of a television circuit must be regarded, however, as a mathematical method of expressing the response not directly related to the physical conditions of the problem. It is true, of course, that the signal produced by scanning a fixed picture is periodic with a fundamental frequency of 25 cycles per sec., but this does not alter the fact that the eye is primarily aware of the time response of the circuit and not the frequency response.

Frequency response is still of great value in analysing mathematically the behaviour of different circuits and relating this to measurements on the circuit, as shown by the present paper, but the point which I think needs to be stressed is that in order to achieve a given quality of transmission we must stipulate, at first, not the frequency response but the time response of the circuit, as is done by the authors.

The conditions which must be applied to the time response of a television circuit must be based fundamentally on the maximum rate of change of spot brightness required to achieve a given definition, and on the minimum perceptible percentage difference of brightness, or perhaps, in some cases, the maximum tolerable percentage difference of brightness in regions which should be of uniform brightness. For the minimum perceptible percentage difference of brightness, I am interested to note that the authors suggest a value of 2 % of the maximum brightness. The minimum perceptible percentage difference of brightness is, according to Fechner's law, nearly constant over a wide range of brightness and equal to about 1 % to 2 %. We find that these figures are confirmed by tests of the interfering effect of additional unwanted signals.

Referring to Fig. 33, I note that the impedance/frequency characteristic of the aerial-feeder system showed a variation of $\pm$ 5 ohms in 78 ohms over the frequency range 43 to 47 Mc./sec. As is explained by the authors in Section (2), this means that the radiated energy will vary over a range of $\pm$ 0·5 db. for constant input voltage to the output stage of the transmitter over this frequency range. The overall fidelity of the transmitter over the *video* frequency range 0 to 2 Mc./sec. will therefore probably vary over about the same range, assuming perfect response in the modulator over this range. I should be interested to know whether any tests have been made to confirm this.

Mr. T. H. Bridgewater: My few remarks will be confined to the paper by Messrs. McGee **and Lubszynski.**

The Emitron is a very remarkable device because it seems to have been developed on the basis of the frame storage theory, and then to have been found to work very well but according to quite a different explanation, namely the line sensitivity theory. Perhaps I may be permitted to make a few remarks about the working conditions and behaviour of the Emitron at Alexandra Palace.

The beam current which is generally employed is between 0·05 and 0·15 μA. The spectral response of the mosaic appears to correspond very closely to that of the human eye, such variations as occur between one Emitron and another, or during the life of a particular Emitron, being found towards the red end of the spectrum.

For studio work, the mosaic generally has an illumination of between about 0·75 and 1·5 foot-candles. This seems to be satisfactory for most subjects, except for certain kinds having large black areas, particularly in the lower portion and extending to the borders. When these black areas occur it is sometimes necessary to increase the illumination, particularly on the dark parts mentioned, in order to overcome distortion which shows itself as a rather intense white edging to the black parts, encroaching to some extent beyond the edges of these.

Another form of distortion which is noticed from time to time, generally called "streaking," is the appearance of a white overshoot from a horizontal black area on a light background, or a black overshoot from a white area on a dark background. This effect apparently occurs with all Emitrons, and varies in amount according to the contrast ratio, the degree of illumination, and the beam current. It can be corrected for electrically, provided no variations occur of the controlling factors mentioned. Another common effect consists of a darkening of the top of the image which extends for 10 % or more of the picture height, and varies as between one Emitron and another and during the life of a particular Emitron. Perhaps the authors would explain in a little more detail the causes of these distortions.

Mr. E. K. Sandeman: I notice that Messrs. Cork and Pawsey found it necessary to use a thermostat to control the temperature of the terminating resistance; what steps did they take to control the resistances and impedances used in the substitution method which they adopted? This substitution method is rather novel; was its use decided upon because the authors found difficulty in connection with the conventional bridge method at such a short wavelength?

The authors state that a deviation of $\frac{1}{20}$ in. in the centre conductor induces an impedance variation of 9 ohms, which they consider too much. Apparently they eliminate such variations by rotating parts of the centre conductor which are out of truth, and by various other means. It does not appear to be hard to make a mechanical device true to $\frac{1}{20}$ in., and I should like to know whether any particular difficulties have been experienced in doing this.

Referring to the paper by Messrs. McGee and Lubszynski, it would be interesting if the authors could add a diagram illustrating the variation of sensitivity of the various types of Emitron throughout both the luminous spectrum and the infra-red region.

Mr. W. H. Ward: When television becomes one of our major industries it will be necessary to draw up British Standard Specifications for television cables, and before this can be done a standard test will have to be evolved. As this test will have to be applied by men who are not at present familiar with high-frequency technique the committee which has to draw it up will have a difficult task.

I gather from Mr. Birkinshaw's remarks that the Alexandra Palace feeders vary considerably in insulation resistance with the weather but that their behaviour in their working conditions remains the same. It is possible that this may be explained by the large variation, with humidity, of the power factor of steatite at low frequencies as compared with high.

The depth of penetration of a current at frequencies such as that of the Alexandra Palace transmitter is quite small, and therefore any dirt or copper oxide on the surface of the copper conductors might have an appreciable influence on their effective resistance, and consequently on the attenuation.

I notice that Messrs. Cork and Pawsey have succeeded in using $\frac{1}{2}$-watt metallized resistors as sub-standards. Does it just happen that resistances of about the values which are wanted are nearly independent of frequency? Has there been any serious difficulty in dealing with the reactance of these resistors?

In measuring the constants of the circuit, the unknown impedance is determined from a ratio of two voltages. I know from experience, however, that this method is by no means always justifiable. Can the authors give any hint as to how discrepancies may be avoided?

Finally, various parts of the authors' apparatus are screened by copper sheet, which may not be an equipotential surface at the frequencies in question. It would be interesting to know whether this factor has caused any trouble.

Mr. R. F. O'Neill (*communicated*): The paper by Messrs. Cork and Pawsey explains the importance of minimizing reflections due to irregularities in the feeder system of a high-definition television system. Since in visual reception the effects of reflection are in general more noticeable than in the case of aural reception, the requirements, as regards uniformity and correct matching, imposed on any cable link in such a system are more stringent than in the case of sound transmission.

The problem of overcoming distortion from this cause has also received considerable attention abroad, notably in Germany, where television signals are relayed by cables. The work of German investigators has been chiefly concerned with the question of random irregularities in the cable and in establishing permissible mechanical tolerances in manufacture.

Picture distortion due to echoes was also encountered in the development of facsimile equipment some 10–12 years ago, in the course of transmissions over long trunk routes. This distortion, which came to be known as "plastic," was accounted for partly by reflection and partly by phase distortion, due to non-linear phase/frequency characteristics; and to reduce these effects specially corrected cables are now employed for commercial picture transmission. An excellent treatment of the subject of "plastic" is given by Dr. Schröter in his book on picture telegraphy.*

Irregularities in cables have of course engaged the attention of telephone engineers for many years. In telephone transmission they impose a limitation on the accuracy of balance which can be maintained between the cable and the balancing network in two-wire repeater working. In this connection, attention may be drawn to a paper by Dr. Rosen,† and it is interesting to note that his analysis of the effect of one or more discontinuities is identical with that of the present paper. Among the deductions made at the end of Dr. Rosen's paper is that the extent to which neighbouring deviations in opposite directions cancel each other depends upon α, and, other things being equal, the greater the phase constant the more will be the resultant irregularity at the end.

As will be realized from the paper, much of the preliminary investigation, and its consummation at Alexandra Palace, was in the nature of field work, and the good agreement shown between theory and observation is ample testimony to the accuracy of the authors' measuring gear.

Mr. K. S. Phillips (*communicated*): Messrs. McGee and Lubszynski describe how, in the case of the Super-

* F. Schröter: "Handbook of Picture Telegraphy and Television" (Julius Springer, Berlin, 1932).
† *Journal I.E.E.*, 1927, vol. 65, p. 989.

Emitron, the electron gun is not situated below the window of the camera, but is rotated about the axis of the window neck, to compensate for the rotation of the image due to the magnetic lens. Mr. Espley suggested at the discussion that it would lead to simpler construction if the scanning system were rotated about the electron gun, and the gun itself situated below the window, as in the ordinary Emitron. While this would mean that a complicated scan would be required, it must be remembered that, except when the gun is situated behind the mosaic, a trapezium scan must always be employed. If the retention of the electron gun exactly below the window does in fact represent an advantage, there appears to be a better method of achieving this.

Consider an electron lens in which the magnetic field along the axis at x is Hx, and the accelerating potential is V. The focal length of the lens is given by

$$\frac{1}{f} = \frac{0\cdot 022}{V}\int_{-\infty}^{\infty} H_x^2 dx$$

The angle ψ, through which the resulting image is rotated, is given by

$$\psi = -\frac{0\cdot 15}{V^{\frac{1}{2}}}\int_{-\infty}^{\infty} H_x dx$$

Now the sense of H_x depends on the direction of the current in the focus coil. If, then, we employ, instead of one focus coil, two adjacent coils in which the focus current flows in opposite directions, the focusing effects of the two coils will add up, while rotational effects will subtract. Hence we can arrange that the total rotation is nil. This idea was suggested by Stabenow.*

Mr. N. M. Rust (*communicated*): Messrs. Cork and Pawsey quite correctly state in the Introduction to their paper that the conditions necessary to ensure the distortionless transmission of a television signal through a feeder system have received little attention. They do not bring out, however, that the effects involved were previously well understood in relation both to telephone and to facsimile transmission along wires, and to some extent in relation to high-frequency feeder applications for commercial communications. The measurement technique employed was also a special adaptation of the well-known "impedance meter" technique used in the adjustment of commercial short-wave transmitting and receiving systems, which had the advantage of simplicity and robustness, and gave sufficiently accurate results to fulfil the requirements involved.

Comparing short-wave communication problems with television, in commercial short-wave applications the highest carrier frequency used is approximately half the television carrier frequency, whilst the sideband spectrum required even for telephony is only $\pm$ 10 kc./sec., in contrast with the $\pm$ 2·5 Mc./sec. required for television. In consequence, effects which may be of importance for television are of no practical importance for commercial communications. In quite early work with Franklin feeders it was found that the half-wave spacing of insulators must be avoided as giving undesirable effects. The quarter-wave cancellation effect was also appreciated, although it was never necessary to make practical use of it.

In some cases it was found to be desirable to correct for expansion-box irregularities. This was carried out by the very simple and practical expedient, from the point of view of the installation engineer, of using a larger-diameter inner conductor in the expansion box to bring its characteristic impedance (treated as a short length of line) down to that of the feeders which it connected together.

In relation to the brief description of the aerial system, although it is appreciated that this was from the authors' point of view regarded as incidental to the method of transformation and correction employed in matching it to the feeders, it would certainly have proved fitting to have mentioned Mr. C. S. Franklin as being responsible for its design and development.

I should like to say that the authors displayed great patience and courage when carrying out these measurements. Even in comparatively calm weather there was quite perceptible sway in the experimental mast at Hayes and in that at Alexandra Palace. In the latter place the aerial measurements were carried on considerably more than 200 ft. up in the air, and in some cases the support for both apparatus and personnel was rather precarious. Difficulties were added in bad weather owing to the slippery foothold on the metal ladders and platforms. It will be realized to some extent what conditions were like if it is mentioned that safety belts had frequently to be used.

THE AUTHORS' REPLIES TO THE DISCUSSION

Messrs. E. C. Cork and **J. L. Pawsey** (*in reply*): Mr. Bedford has raised the fundamental point as to the degree to which the feeder and aerial must be free from reflection phenomena. The problem arose from the observation of striae in pictures due to reflections occurring in what would be considered to be a reasonably broad-band aerial system. The aim of this work was to reduce this form of distortion below the limits of visibility, allowing for future improvements in definition of receivers up to the useful limit of the system. Estimates along the lines of the paper indicate that the permissible degree of mismatch is small. Observations on high-definition receivers using long low-loss aerial feeders have frequently shown this form of distortion, which disappears when a broad-band matched-aerial feeder system is used. With regard to the possibility that the accuracy of the results given may be of an artificial nature, the fluctuation of resistance with frequency constitutes a fairly direct measure of the reflected waves arriving at the measuring point. The absolute measure of the input impedance is relatively less accurate than the fluctuation, but the final results do not depend on any absolute value of impedance. Indeed, a completely satisfactory definition of the impedance between two points separated by a finite distance appears to be lacking.

We are interested to learn from Mr. Birkinshaw that

* *Zeitschrift für Physik*, 1935, vol. 96, p. 634.

the insulation resistance of the feeder maintains a high value. Great care was taken as to the scrupulous cleanliness of the insulators and the sealing of the angle boxes. Without such precautions large fluctuations can take place. In the course of the erection of the feeder various slight mismatches were taken up by the methods indicated in the paper.

In reply to Mr. Espley, the feeder selected was one of the standard types available, and has an ample margin of safety. There is no significance in the radius ratio 3·6 in this case. The effect of the proximity of the sound aerial was measured, the voltage induced into the vision aerial being about 1 % of that in the sound aerial. With regard to series resistance developing in expansion joints, we were apprehensive of this trouble, but with the type employed no faults due to this cause were encountered. We have no information as to the steady-state propagation characteristics of the transmitter.

With regard to the reduction of the effects of eccentricity discussed by Mr. Mumford, the necessary insulator clearance and consequently the maximum eccentricity is determined by the possible ellipticity and lack of uniformity of the pipes, so as to ensure that the insulators are always free. Probably in a future installation sufficiently uniform pipes will be obtainable to allow considerably smaller clearances. Small correcting condensers were occasionally inserted in the Alexandra Palace feeder to correct for eccentricity.

With reference to the degree of maintenance of the characteristic curves of the feeder and aerial, a check measurement made about a month later showed no appreciable change. Further, since observations of the transmitted picture have shown no evidence of distortion due to this cause, it is probable that the system has remained substantially unchanged. As Mr. Mumford has pointed out, the aerial produced a mismatch much greater than that due to irregularities in the feeder. In the initial experiments a centre-fed dipole aerial of about 2 ft. diameter was developed and erected about 15 ft. above the top platform of the Hayes mast. The resistance variation over 4 Mc./sec. was 24 %, and the reactance variation 20 % of the tuned resistance value. When this aerial had been corrected by a suitable tuned circuit the maximum impedance deviation from the characteristic impedance was $\pm 2 \cdot 5$ %.

We agree with Dr. Jarvis as to the fundamental nature in television of the time response of circuits as opposed to frequency response. From this point of view the analysis of a circuit may both practically and theoretically be facilitated by the use of pulse technique. The practical application of such technique requires more elaborate apparatus than the simple measuring circuit indicated in the paper. With regard to the variation of the output of the transmitter, measurements of the overall response have been made over the range of modulation frequencies, using a receiver at a distant point. The variation of the output is complicated by the existence of the two sidebands, which have different amplitudes due to the non-symmetrical form of the overall aerial-feeder characteristic. The measurement is the resultant of a large number of factors, with the result that the overall accuracy is not sufficient to show a variation of a few per cent due to the aerial.

Mr. Sandeman has raised the question of the temperature control of the measuring resistances. These were frequently measured by means of a bridge on the spot and showed little variation. The measuring circuit was adopted after considerable testing and correction for the residual errors of leads. Bridge methods were tried, with the consequent increase of reading accuracy owing to the use of a null detector, but this accuracy can be fictitious and we have not so far replaced the substitution method for general use.

We agree with Mr. Ward that it will be necessary to lay down specifications and methods of testing for high-frequency cables, which are already numerous. Provisionally we specify the cable in terms of its characteristic impedance, attenuation, and velocity constants. It seems desirable to have some figure describing its uniformity and also a comparison with a cable with copper conductors and a loss-free dielectric, probably air. We have no information of the magnitude of the increase of attenuation due to copper oxide. It appears to be a fortunate accident that resistances of the type used were approximately independent of frequency up to at least 50 Mc./sec. Further, they are substantially resistive, the change-over between the capacitive reactance of very high resistances and the inductive reactance of very low ones occurring somewhere in this region. Errors may arise from several causes in deducing resistance from a ratio of voltages. Those due to the unknown law of the voltmeter and the change of loading on the oscillator are evaded by the substitution of an equivalent resistance. Errors arising from the reactance of leads must be reduced to small magnitude in the design of the apparatus. Considerable errors may arise from potential differences over the surfaces of metallic sheets, on account of the finite impedance of the surface. These potentials may be reduced by adequate screening and the design of the apparatus to avoid induced e.m.f's.

We thank Mr. O'Neill for pointing out Dr. Rosen's treatment of irregularities in cables, which is a fuller treatment of Section (3) of the present paper.

In reply to Mr. Rust's remarks, of course reflection problems in long telephone lines have been dealt with for many years, and cases have arisen—particularly in long-wave aerials, where the band-width is a reasonable percentage of the carrier frequency—requiring very careful building-out of the aerial impedance; but as far as we know this is the first occasion on which the double problem has been dealt with, where not only is the band-width a sufficient percentage of the characteristic frequency to require careful treatment of the aerial, but also the aerial feeder is long with reference to the modulation band-width. With regard to the aerial, we have stated that it was designed by the engineers of the Marconi Co. and are glad to join with Mr. Rust in acknowledging the part played by Mr. Franklin in this work.

Messrs. J. D. McGee and **H. G. Lubszynski** (*in reply*): Replying to Mr. Bedford, the transmission of film is more difficult than the transmission of studio scenes, because of the frequent rapid changes in illumination encountered with the former. These changes require frequent and rapid readjustment of "tilt" correction. The intermittent scanning method for film transmission

was not discarded owing to an insufficient "memory" of the Emitron, but because with this type of scanning large additional undesired pulses appear between frames and complicate the somewhat difficult conditions still further. From this point of view the continuous-motion projector offers more advantages than disadvantages.

In reply to Mr. Birkinshaw, the type of mosaic with low red sensitivity now in use in the Emitron differs considerably from the old type of mosaic described in the paper. This new mosaic will be described in a future publication by Dr. L. Klatzow.

Replying to Mr. Espley, the electron-lens correction for keeping the scanning beam in focus over the whole of the mosaic area was never used in practice because it was found possible to obtain sufficient depth of focus of the electron beam to cover the whole mosaic by stopping down the apertures which limit the electron beam. The method could, however, be used in both frame and line direction.

The compensation of the image rotation by rotating the deflecting fields would complicate the scanning circuits. Owing to the oblique incidence of the scanning beam on the mosaic, the scanned patch shows a keystone distortion when the line-scanning amplitude remains constant over the whole frame period. This distortion is compensated electrically by increasing the amplitude of the line-scanning currents from the top to the bottom of the mosaic. If the deflection fields were rotated a second compensation would have to be introduced in the frame-scanning circuits, which would complicate matters considerably.

The spot of low sensitivity which sometimes appears in the centre of the picture is due to burning of the sensitive surface by the electron beam, which has been allowed to remain stationary for some time. Safety devices are now used which cut off the electron beam if the scanning fails.

No difficulty is experienced due to "poisoning" of the mosaic by negative ions if satisfactory vacuum conditions are maintained.

In reply to Mr. Mumford, the noise in the picture from an Emitron or Super-Emitron is composed of three components: (1) the noise of the first stage of the amplifier; (2) the noise carried by the scanning beam; and (3) the noise of the photo current. Of these, the first is the determining factor. The noise contributed by the scanning beam is, at present, about one order of magnitude below the amplifier noise. The signal output must be so large that the signal is not drowned by the noise of the amplifier. Under these conditions the noise of the photo-current is negligibly small. When reference was made to the sensitivities of the two types of tube, their signal output in volts at equal light flux into the tubes was meant. Therefore, the statement that one type of tube is n times more sensitive than the other means that its signal/noise ratio in volts is n times better for equal light flux. As in these tubes the signal output is proportional to the light input only up to a certain intensity of illumination, and as the signal reaches a saturation value for strong illuminations, it is more correct to state that if one type of tube is n times more sensitive than another their signal/noise ratios in volts are equal when the light input into one is n times smaller than that into the other.

The signal output can be increased by increasing the beam current, and consequently the signal/noise ratio could be improved considerably before the noise due to the scanning beam became serious, but with increasing beam current the "tilt" signal becomes unmanageable without elaborate correcting circuits. Thus the spurious "tilt" signal ultimately sets the limit to the signal/noise ratio obtainable.

The inherent "tilt" is approximately the same for the same beam current in both types of tube, and hence their signal/"tilt" ratios are to one another as their sensitivities. The greater signal/noise ratio of the Super-Emitron is mainly due to the more efficient conversion of the light image into an electrostatic charge image on the mosaic.

The Super-Emitron is not used universally by the B.B.C., for several reasons. The few cameras and tubes at present available are largely experimental and it was considered inadvisable to make a large number of either until they had given convincing proof of their superiority over standard Emitrons, of which a large stock must be kept on hand. Consequently the Super-Emitron cameras have been used mainly for outside broadcasts which would have been difficult, if not impossible, for the standard Emitron. In these cases the geometrical distortion of the picture transmitted by the Super-Emitron was tolerated in order to get a picture at all, while the standard Emitron with its more accurate geometry of picture is preferred for studio work, where adequate illumination can be provided. Improvements in design which are being introduced into new Super-Emitrons will, we hope, enable them to displace the old cameras from the studio, with considerable improvement in the pictures.

Dr. Gabor's objection to accepting our interpretation of Campbell Swinton's suggestion appears to be based on the erroneous impression that Campbell Swinton had in mind only photo-conductive mosaics. That this is not so is clear from his address to the Röntgen Society,* in which he describes a mosaic as follows: "The metallic cubes which compose J (the mosaic) are made of some metal such as rubidium. . . ." This would be a photo-emissive mosaic which would have no time-lag and would be capable only of charge storage.

Pincushion distortion is not a defect of electron images formed by electromagnetic electron lenses. Hence no correction is necessary.

With regard to the secondary emission from carbon, the maximum secondary emission from pure carbon is very low indeed. It was found, however, that carbon adsorbed caesium very readily, and then secondary coefficients as high as 9 secondaries per primary were obtained.

In reply to Mr. Bridgewater, all the types of distortion described are particular forms of "tilt." These spurious signals depend on several factors, such as the distribution of illumination on the mosaic, the nature of the mosaic surface, small stray magnetic fields, and so on.

The type of distortion which shows as a white edging extending into large areas of black in the picture is due to the "tilt" signal assuming a wave-form which cannot easily be corrected by the simple correcting circuits that

* *Journal of the Röntgen Society*, 1912, vol. 8, p. 11.

are used. The image can be improved by increasing the illumination, since then less amplification is required and a better signal/tilt ratio is obtained.

The dark band which sometimes appears at the top of the picture is due to an accentuation of the strong black pulse which occurs at the beginning of each frame. When the scanning beam begins a new frame, it is surrounded by a region of the mosaic which is at a more negative potential than normal. Hence the secondary electrons liberated by the beam tend to return to the collecting electrode in greater numbers than usual. Thus, although picture signals are produced the black level is depressed, and it is only after the spot has scanned a number of lines and so built up a normal distribution of potential around it that the black level of the picture reaches its normal value. This edge effect is usually suppressed in the amplifiers, but occasionally under unfavourable conditions it may extend into the picture. To reduce this effect to the minimum the scanning beam should be allowed to start scanning the mosaic immediately it has completed its return stroke, so that it may have as much time as possible to build up the normal charge distribution on the mosaic before the amplifier suppression is removed.

The type of distortion known as " streaking " may be explained as follows. The production of undistorted picture signals depends on the assumption that the peak positive potential, to which the elements of the mosaic are brought by the scanning beam, is constant and the same at all points of the mosaic, and that a constant fraction of the secondary electrons liberated from a point return to the second anode. That this is not strictly true is shown by the appearance of spurious signals such as " tilt." It is clear that the peak positive potential to which the mosaic elements are charged cannot vary rapidly, otherwise fine detail of gradation in the image would not be reproduced. Hence it should follow that when the electron beam scans a positively charged (white) part of the mosaic, the peak potential will rise a little higher than when it scans an uncharged area. While such areas alternate rapidly, they will have very little effect on the peak potential of the scanned spot. If, however, the spot remains on such a charged area (for example, a white band running parallel to the scanning lines across the picture) for a considerable part of one line, the peak potential will drift more positive and the amplitude of the signal will decrease. When the scanning spot passes off the white band on to the black surrounding area it will initially produce a signal of too great an amplitude in the black direction which fades out in approximately the time taken to scan one line.

The comparatively slow rate at which this peak potential drifts under the influence of charged areas on the mosaic is probably due to the control exerted on the potential to which each mosaic element will rise by the potential of the element that has just been scanned; that is, the most positive area in the neighbourhood. This tends to keep the peak positive potential constant, and it is only slowly influenced by charged areas on the mosaic.

In reply to Mr. Sandeman, the maxima of the spectral response curves of the Emitron and Super-Emitron are in the yellow-green, and the whole response curves are very close to that of the human eye. There is very little sensitivity in the infra-red. Both types of tube can, however, easily be made so that they have their maximum sensitivity in the red, and their response extending in the infra-red to about 12 000 Å.U.

Replying to Mr. Phillips, the two-coil method of obtaining images without rotation, suggested by Stabenow,* was tried but was found unsatisfactory. Though an upright picture was obtained over a small central zone, the spiral distortion at some distance from the axis was so enormous that the rest of the image was spiralled into a bright ring surrounding the central area.

* *Zeitschrift für Physik*, 1935, vol. 96, p. 634.

DISCUSSION ON "THE MECHANISM OF THE LONG SPARK"*

BEFORE THE INSTITUTION, 3RD NOVEMBER, 1938

Sir George Simpson: A great deal of knowledge has been gained of the mechanism of sparks at atmospheric pressure since I attempted to work out the physics of a lightning discharge in 1926. Schonland's discovery of the "leader stroke" was an important step forward, and the clue he then gave has been fruitfully followed in the laboratory by the present author. There can be no doubt that a leader stroke plays an important part in the mechanism of a long spark discharge. The leader blazes a trail which is followed by the real discharge when an ionized path connects the two electrodes. A spark would then appear to consist of two successive phases: the leader and the return stroke. I am afraid, however, that this is not the whole story, for while it explains a large proportion of lightning strokes there are others which do not appear to fit into this simple mechanism, and I propose to say a word or two about these exceptional discharges.

There is a type of lightning discharge which was called by W. J. S. Lockyer "stream discharge." A discharge of this nature leaves the cloud and travels many miles, mainly horizontally, before finally reaching the ground. But the outstanding features are that it shows no tendency to branch and is of the same intensity throughout its long length. It is impossible to believe that a discharge of this nature can be composed of a leader stroke and a return stroke.

Then there is the rocket flash, which, as its name implies, travels so slowly that its progress through the sky can be followed by the eye just as the path of a rocket is followed. It is impossible to believe that a flash of this nature which takes probably a second to transverse a few miles is preceded by a leader flash.

But of all flashes which are difficult to explain by the leader mechanism the meandering flash takes the first place. A photograph which I have of such a flash shows it as having literally tied itself into knots. A large part of this appearance is probably due to perspective and to there being more than one discharge. But the path is so involved, in places turned on itself, that the leader mechanism as described by Schonland and Allibone can play little part in its formation. In another photograph the flash appears to have meandered to the surface of the sea and then risen and entered the cloud again.

Finally, there is the celebrated "ball lightning," the objective reality of which was acknowledged by Goodlet† last year. There can be no doubt that this is some form of electrical discharge; but the mechanism of its formation must be entirely different from that of other lightning discharges.

Prof. W. M. Thornton: The author says rightly that an immense amount of research work has been done on the short spark but very little on what he has called the long spark, that has all the characteristics of miniature lightning. One very good reason for this is that most of the laboratories with plant suitable for high-voltage research are so fully engaged on more directly industrial problems that facilities for work of this high order are not readily available.

I regard the paper as an attempt to solve by controlled experiments some of the very complex problems that are still obscure in lightning discharge, the oldest of all electric transients. In the short spark, branched paths have not time to develop; in lightning they are not under control. Perhaps the most interesting feature is the cause and nature of the time-lag that is found, in one form or another, in all spark discharges. Why, for instance, should there be a lag of 300 microseconds between the leader stroke and the main discharge in lightning and only 6 or 7 microseconds (Fig. 6) in the laboratory? The leader stroke is like the sudden failure of the wall of a canal, which empties the channel in its immediate vicinity and is followed by a huge rush of water from both sides towards the place, resulting in a burst of flood from it. Without some retardation of flow of the charge towards the point of failure there would be no interval of time between the leader and main strokes. It is well known that only a small part of the surface region of a cloud is discharged in a lightning flash, and the quantity discharged is very small. Even if the current were 50 000 amperes flowing for 100 microseconds the quantity would only be 5 ampere-seconds, and this is a high value. Quantities of the order of 1 coulomb are usual in lightning. A cloud cannot be regarded as a good electrical conductor. How does the quantity for the main stroke reach the point of discharge; and what happens if the gradient of the leader stroke is itself a transient, as in the Lodge B spark?

In certain rare cases of lightning discharge, fireballs or globular lightning are formed. I should like to ask the author whether he has ever seen an approach to the luminous globe of gas, mostly ozone, that has been known to form under laboratory conditions. The late Prof. Clinton told me that on one occasion he had seen one form and then explode with great violence.

Has the author ever observed a multiple leader stroke from the points in the conditions of Fig. 4? There is evidence of corona from many of them, but not a distinct multiple spark. There is one such case on record in the photograph of a lightning flash taken on the seafront at Dieppe: this shows a single thin line of light starting from the top of every lamp post in view.‡ Was this a multiple leader stroke that could not develop with a

* Paper by Dr. T. E. Allibone (see vol. 82, p. 513).
† *Journal I.E.E.* 1937, vol. 81, p. 10.

‡ The photograph was reproduced in the *Electrical Times* of the 5th December, 1907. The points of light are the gas lamps along the promenade.

main stroke because the local concentration of charge had been relieved over a wide area, and there was no rush towards a single channel?

There is one other question I should like to ask the author. We know that electrons when in motion spin. Is there any evidence that the long spark spins on its axis? Does every spark spin, and how can the spin be observed? Has the spectrum of such transient discharges ever been examined? Does the spectrum of shock excitation differ in any way from the spectrum of continuous discharge usually employed in the spectroscopic examination of gases? The answer to this question might give a clue to whether the spark spins as a whole and to the rotatory nature of an electric field.

Dr. E. H. Rayner: The paper invites physical speculation upon a new extension of knowledge of electrical discharge. There has been a great deal of discussion during the last 2 000 years about lightning, and Lucretius gives a very graphic and scientific description of it. I am not sure that in his time, the 1st century B.C., copper lightning conductors were not used, because he mentions in his poem that copper and gold can be melted by lightning.* In a subsequent passage† he amplifies a similar statement by what forms an exact description of modern ideas of ionization by collision.

Referring to Fig. 5A, the author says that "the discharge proceeds in the direction away from the point electrode." This description possibly masks the physics of the process. I imagine that the effect of the application of a positive charge to the metal is to draw towards it the negative electrons in the neighbourhood, and these, moving with enormous velocity, ionize the air. Electrons a little farther away are drawn into the conducting channels in the mass of gas, which immediately becomes positively space-charged. The pattern in Fig. 5A thus grows from the point outwards, the point drawing into itself the negative electrons in an intense stream, which extends backwards like the thumb and fingers. It will be noticed that the streams do not cross each other, although the smaller branches may finish on one of the main streams. The method of photographing shown in Fig. 5A seems to me to give a better insight into what is happening than the ordinary kind where a photographic plate is put at right angles to the point.

It might be possible to interpret the negative figures in a similar manner, but they are artistically of so much less interest that one is inclined to neglect them. It would be interesting to know whether the author can put any similar interpretation upon them. In negative figures the accelerating agent, i.e. the positive field, is not on a solid surface, as in the other case, but is contained in the space around. Possibly this fact may give us an insight into what is happening. The actual sharp boundary to the most active part of the record of the negative discharge must have some physical importance; it would be interesting to know how much of the effect, both of the positive and of the negative photographic result, is electrical and how much is due to luminosity.

With regard to the preponderance of strokes in one direction, some few years ago at the National Physical Laboratory on our Visitors' Day we demonstrated the effect of the erection of a small point on a plane electrode. We showed what the present author has demonstrated, namely the attraction of the discharge to the point in one direction and the complete neglect of the point in the other.

Dr. W. Wilson: The author has performed a valuable service by establishing the relationship which exists between lightning and a laboratory spark. Lightning treats this country very shabbily; its visitations are infrequent and comparatively mild. Research work on lightning is in consequence handicapped, and this is illustrated by Sir Charles Boys's experience. He invented the camera with which the records reproduced in the paper were for the most part taken, and then waited for a suitable flash of lightning to come and be photographed. The lightning did not come, however, and it was not until nearly half a century later that his apparatus assisted Dr. Schonland in South Africa to make important discoveries. Investigations with artificial lightning will help to remove this disadvantage.

It would be of interest if the author could go a step further and say something as to the relationship between lightning and the Lichtenberg figure or klydonogram. As far as I can see, a klydonograph simply records a discharge which is passing across an insulating surface. Exactly how does the insulation modify the effect? It is there primarily, I think, as a recording means; by sprinkling it with sulphur or coating it with a photographic film, one can ascertain the path of the discharge. May it not be possible, by attaching the film to the surface of a rapidly revolving metal drum instead of the fixed plate, to obtain analogous effects to those given by natural and artificial lightning?

In papers on this subject, I have not seen a reference to the shape of the lightning discharge. It usually seems to be assumed that lightning is a flow or streak of some kind. The rotating-camera records, however, would seem to indicate that the discharge resembles a sphere, otherwise the width of the photographic track would vary with its inclination. I should like to have the author's opinion as to whether lightning does not consist of a species of spherical "projectile," having a diameter of, say, 4 in. to 6 in. for the leader strokes and 2 ft. to 3 ft. for the main strokes.

Sir George Simpson, in his Kelvin Lecture,* proposed a very neat and interesting theory with regard to large and small strokes. These used to be termed "direct" and "induced," but he expressed the opinion that an induced stroke of any magnitude was an impossibility. The theory was that when a discharge branches upwards, a large portion of the energy in the cloud is concentrated in one spot on the earth, producing a large stroke; but when the discharge branches downwards, the charge from a small portion of the cloud is spread over a wide area, and its effect is far less severe. This hypothesis agrees very well with the effects experienced, and I should like to ask the author whether it still holds good.

* *De Rerum Natura*, VI, 230:—
"Et liquidum puncto facit aes in tempore et aurum."

† *Ibid.*, VI, 352:—
"Dissoluit porro facile aes aurumque repente
Confervefacit, e parvis quia facta minute
Corporibus vis est et levibus ex elementis,
Quae facile insinuantur et insinuata repente
Dissoluunt nodos omnis et vincla relaxant."

* *Journal I.E.E.*, 1929, vol. 67, p. 1269.

Dr. S. Whitehead: I should like to discuss certain of the differences between discharges in relatively uniform and symmetrical fields and those in divergent and asymmetrical fields. It is convenient to consider this question in relation to the sphere-gap, which can show a regular transition from one type to the other.

At small spacings the field is homogeneous and symmetrical. The author has mentioned that Townsend's mathematical treatment is valid for two or three different types of ionization processes additional to electronic ionization by collision. Townsend's formulae, as modified by Schumann, lead to the expression that the integral of the ionization coefficient over a path between the electrodes—i.e. the number of first-order ions formed by an electron—is a constant. If that integral is applied to a uniform field, using the formula $\left(\frac{X}{p} - \text{Constant}\right)^2$, given on page 514, this formula yields the correct voltage/spacing law as found by Toepler and Stephenson. Since this derivation does not allow for distortion by space charges, it seems reasonable to conclude that these are not developed until the voltage has reached the minimum sparkover value. As the sphere-gap is opened out, the field becomes somewhat divergent, while an asymmetry is introduced if one sphere is earthed. As I hope to show in a paper to be published shortly,* for spacings which do not exceed a radius this asymmetry is too small in practice materially to affect the sparkover voltage, either positive or negative, which remains the same (1 % to 2 %) whether one sphere is earthed or the voltage is symmetrical. The divergence of the field, however, reduces the sparkover voltage as compared with a homogeneous field by an amount corresponding roughly to that calculated from the Townsend–Schumann formula.

If the spacing is still further increased, the asymmetry due to earthing one sphere becomes greater, and both positive and negative sparkover voltages are decreased, but the negative is decreased much more than the positive. Thus for spacings up to 1 or 2 diameters negative sparkover is less than positive sparkover, while for greater spacings where the asymmetry between the electrodes is greater the positive sparkover is less than the negative, and we finally reach the point-plane gap dealt with by the author. In addition, at spacings between about 1 and 3 diameters corona makes its appearance before sparkover occurs, and above 4 diameters there is an appreciable difference between the sparkover and corona voltages. Lastly, over the region where the negative sparkover voltage is less than the positive, the decrease in the negative sparkover voltage caused by the asymmetry introduced when one sphere is earthed—obviously there is no polarity effect in the symmetrical gap—agrees more or less quantitatively with the relative increase in the field at the high-voltage electrode which accompanies the asymmetry.

The explanation seems clearer if one regards as fundamental the point-plane type of discharge described by the author and if one also regards as essential the presence of corona or—since the actual discharge may not matter so much—the type of field which is capable of producing corona. This intense localized field yields intense ionization immediately adjacent to the electrode. If this is the positive electrode, then it is naturally easier to form a core of positive ions by withdrawal of the electrons, and this core is also impelled forward like an electric cutting edge or point. On the other hand, when the field is more uniform and when, as we know, the conditions do not produce corona, this local intense field is absent. It is therefore necessary to distort the field in such a way that corona can form, at least theoretically. Now, space charges can do this and the fact mentioned by the author, that a leader can begin from a point between the electrodes, supports the view that the space charge is formed from any convenient source of ionization within the gap. The easiest way for the requisite conditions to arise is by a cloud of positive ions becoming located near the negative electrode, thus giving an intense field at the negative electrode, so that any irregularity has field conditions in its neighbourhood which resembles those of the point discharge investigated by the author. The spark then follows in the manner shown in the author's experiments, as for the negative point-plane gap. In this way it is to be anticipated that the first effect of the asymmetry in a sphere-gap will be to reduce the negative impulse sparkover more than the positive. Further support for this view is lent by the fact that the leader in the homogeneous field starts more often from the cathode. The reversal of sign should accordingly occur more or less where corona begins to precede the spark, which is to some extent the case. Again, for the negative electrode the conditions for sparkover will not be affected by the asymmetry, provided that it increases the field at this electrode, keeping it still the preferred electrode. It is to be expected, therefore, that the decrease in negative sparkover voltage due to the asymmetry will be according to the increase in field at the high-voltage electrode, which fits in with the facts.

Another curious phenomenon, susceptible of a possible but not extremely probable deduction, is that if a wire which is the source of corona is placed near the sphere-gap it may increase the sparkover voltage. This is explicable on the assumption that electrons either migrating from the wire or formed by absorption of radiation in the gap will neutralize the positive space charge and reduce the field. Migration of electrons is supported to some extent by the fact that the effect is said to disappear if there is no metallic connection between the wire and the sphere-gap circuit. On the other hand, ionization of the gas would require multiple absorption of quanta, while the electrons will probably have become heavy and less mobile negative ions by the time that they arrive near the gap, if they ever do so.

A further advantage of considering the corona-spark combination in the way suggested is that the initial conditions for corona apply to the undistorted field. Accordingly, when corona occurs at a definitely lower voltage the corona-voltage law should follow simple equations as derived from the Townsend–Schumann formula, whereas the sparkover voltage should follow quite a different law, since the field for sparkover is distorted. This is in accordance with observation for

* "A Note on Standard Calibrations of Sphere-Gaps," *Journal I.E.E.*, 1939, vol. 84, p. 408.

gaps such as the sphere-gap, concentric cylinders, parallel wires, and so on.

The corona in the divergent field which results from this distortion of the homogeneous field is not stable as with the point-plane gap. The reason for this apparent discrepancy arises from the fact that although for wide gaps the spark-voltage curve falls above the corona-voltage curve, yet the extrapolated portion for small gaps falls rather below that curve. The actual spark-voltage/spacing curve shows a discontinuity at this point. Therefore, provided the initial conditions for corona can be established, there is more than enough voltage to cause the spark to travel across the electrodes. In other words, the gap is essentially unstable.

Dr. J. L. Miller: The three main pieces of apparatus used by the author are the high-voltage impulse generator, the high-speed cathode-ray oscillograph, and the Boys rotating camera. Each of these is in itself sufficiently intricate to require a specialized technique, and all these different techniques—the only thing in common being that they all deal with microseconds—have been successfully welded into a homogeneous whole to produce the results recorded in the paper.

The object of carrying out the work is presumably a twofold one: first, to learn more about the physics of spark formation and propagation; and secondly, to correlate and extend the knowledge gained in order to understand more about lightning.

With regard to the first object, the paper very definitely does extend our knowledge. Like many things that have had academic beginnings it may well have important repercussions in the future of engineering and physics—perhaps in switchgear, rectification, or in some electrochemical process.

With regard to the second object, namely the extension of the knowledge into the realm of real lightning, there are three outstanding considerations. One is the hiatus between the physicist's theories and conceptions of the initiation and propagation of a spark and the engineer's assumption that the lightning stroke may be considered to be a conductor having a surge impedance and upon which exist travelling waves of voltage and current. So far nothing has been done either to link up these two aspects of the spark discharge or to provide an alternative working theory which will help the engineer in his calculations. The second consideration is the understanding of certain aspects of the multiple lightning stroke. Multiple sparks have now been produced in the laboratory; they exist in Nature, and yet we do not appear so far to have measured multiple waves on one conductor of a stricken transmission line. The third consideration is the irreconcilability of the duration of a lightning current in a stricken tower or the lightning voltage on a line conductor and the duration of lightning as determined by radio methods.

Referring to the question of leader strokes, B. L. Goodlet, when he read his paper on "Lightning"* before The Institution fairly recently, stated in his reading that the existence of the stepped leader was a consequence of the nature of conditions at the descending tip of the leader. I submitted in the discussion that perhaps the phenomenon was due to effects at the other end of discharge—that is, in the cloud itself; the effect I instanced was a cascading one between various regions of the cloud. The present author has now produced stepped leader strokes in the laboratory, and from his results he inclines to the opinion that the propagation of stepped leaders in Nature is due to effects in the cloud after all. Apparently streamers have to be propagated from one part of the cloud to the other, and the resulting time-factor permits only a slow rate of rise in potential at the point of origin of the leaders. Thus, the author and I see roughly eye to eye on this point; nevertheless, the Goodlet theory perhaps makes for an easier explanation of the fact that later leaders in a multiple stroke are not stepped. The author's views on this point would be of considerable interest to me.

Regarding the question of the time-lags of spark-gaps, there is little doubt that the time-lag of a long gap is mainly and truly formative. I am not too sure, however, that the time-lag of a small gap having homogeneous electric fields is not also mainly formative; in other words, I feel that the statistical time-lag is often only evident in special cases. For instance, I have had sphere-gaps which when used in enclosed containers—for reasons outside the scope of this discussion—only operated with minimum time-lag when they were irradiated with ultra-violet light. The same minimum time-lag could also be obtained by operating the gaps in sunlight, so that we have a case of a true statistical time-lag being obviated either by irradiation or more simply by operation in sunlight. On the other hand, I have often had the experience of sphere-gaps operating in sunlight with variable long time-lags and these lags were not altered by ultra-violet irradiation or by the presence of radium. Thus the lag here was probably not statistical and yet was too erratic to be formative. I feel, therefore, that there is still further work to be done on this aspect of the subject, and I should be interested to have the author's view on this other side of the problem.

Finally, I should ask whether the same system of potentiometry was used as that described in the author's recent paper in the *Proceedings* of the Royal Society;* in other words, was an antenna used, and, if so, what precautions were taken to ensure that unwanted fields were not included in the measurements? To go further, I would say that perhaps it would have helped the reader of the paper who is not familiar with high-voltage technique if some brief details of the measuring equipment had been included.

Captain B. S. Cohen: I wish to refer to a recent investigation dealing with discharges through films, based on some experiments of 30 years ago. The technique is to use a 50 000-volt direct current, discharging through a film on the surface of a sheet of bakelite. The film consists of gelatine solution incorporating silver iodide and some titanium oxide to give a white background to show up the discharges. The discharges, whilst somewhat of the nature of the Lichtenberg figure, consist of carbonized gelatine following ionization paths. The discharges develop from both electrodes, but generally

* *Journal I.E.E.*, 1937, vol. 81, p. 1.

* T. E. ALLIBONE and J. M. MEEK: *Proceedings of the Royal Society*, A, 1938, vol. 166, p. 96.

start from the negative electrode, and ultimately meet. A line drawing traced from a typical discharge will be found on page 239 in my Address to the Meter and Instrument Section. It will be observed that the positive tracks are different from the negative. In addition, the positive track is surrounded by a nebulous dark margin which could not be illustrated in the line drawing and of which there is no trace on the negative track. Independent discharges sometimes manifest themselves in the area between the electrodes, as illustrated also by Dr. Allibone in some of his photographs. The ionization process in the gelatinous medium has a time-lag of the order of 10^6 compared with gaseous discharges. The former therefore meander along quite slowly; they are accompanied by a sort of wave-front of very vivid balls of white fire. The author tells me that he has found the results of similar discharges on the interior of high-voltage insulator bushings, but has not seen them in course of production.

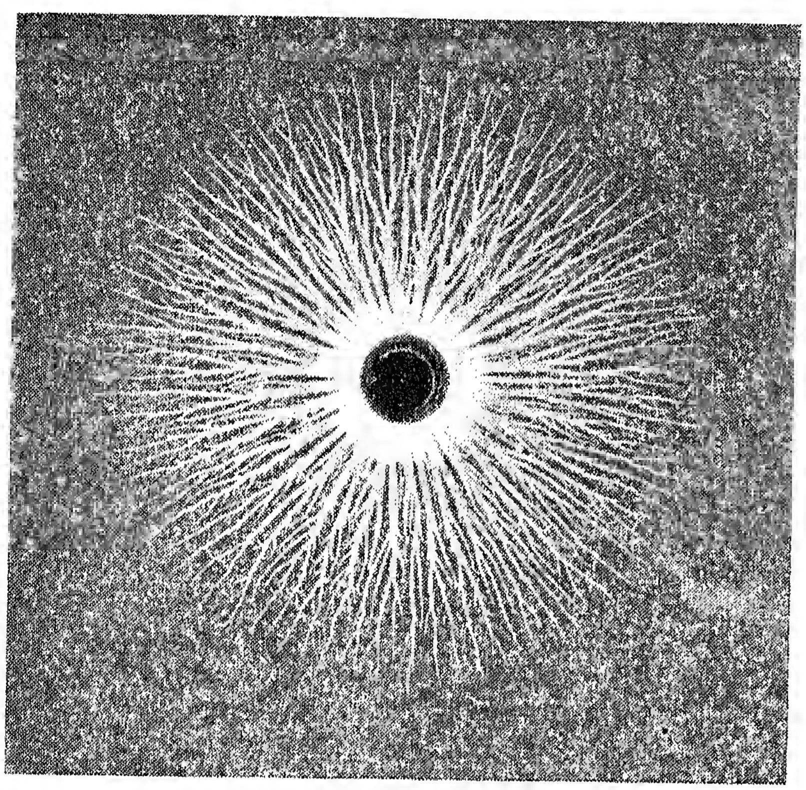

Fig. A

Mr. E. O. Taylor: We have been doing some work on the klydonograph, and have observed the phenomenon shown in Fig. A. This typical klydonogram, obtained from a surge generator when discharging, shows two figures superimposed. It seems that there may be some connection between the positive figure at the centre and the leader-stroke phenomenon referred to by the author, the phenomenon occurring either on the klydonograph discharge itself or else in the sphere-gap which initiates the impulse on the surge generator.

The stepped leader stroke in the lightning discharge is understood to result from the insulating nature of the cloud making it impossible to maintain sufficient charge for the stroke to go right across at one time; but it would not appear at first sight that a similar condition should hold in the laboratory. I should like to ask the author whether there is any possibility of the voltage at the electrode dropping on account of the inductance of the leads between the surge generator and the electrode. Was the oscillogram which he showed taken at the surge-generator terminals or at the electrode, and is there any difference in voltage between these two?

Finally, has the author actually measured the current in the leader stroke? He mentions a figure of a few amperes. Is this figure estimated from observation of the spark?

Mr. W. T. O'Dea: Can the author offer any explanation of the horizontal striations which appear on so many of the photographs taken with the rotating camera? It occurs to me that these must represent ionized or ozonized pockets which persist for variable times after the passage of the main discharge. They might join together as the result of particular atmospheric conditions (temperature, pressure, humidity, or state of charge) and, owing to their greater size and persistence when conglomerated, might be the cause of the phenomena of bead and ball lightning.

Prof. J. T. MacGregor-Morris: There are three points which I should like to make. The first is with regard to the lightning photographs obtained by Dr. B. Walter, of Hamburg, in 1903. He photographed a lightning flash simultaneously with a stationary and a revolving camera,* the latter sweeping the horizon. In Dr. Walter's lightning flash, the luminosity clearly lasted longer in some parts of the flash than in others. Many of the author's photographs show the same feature. It is possible that the spectroscope might help in elucidating this phenomenon. Has the author attempted to do this?

Secondly, it would be helpful to many of us if he would give a brief outline of his special arrangement of the Boys lightning camera. Sir Charles Boys in his first camera, developed many years ago, had two lenses which were rotated by hand at great speed with a fixed photographic plate behind them. The next development was to keep the two lenses fixed and spin the plate; exactly the same photographic result is obtained whilst the arrangement is much simpler. The author is using a further modification of this, and it would be interesting to have further details.

The account which Prof. Thornton has given of the production of artificial ball lightning at University College, London, so closely describes what I did myself, during the time I worked in close collaboration with the late Prof. Clinton for many years, that I think I must have been the doer of the deed! It was in 1896 or 1897 that I had a glass tank about 2 ft. square and 2 ft. deep, filled with dilute sulphuric acid, and this was connected to one pole of a 220-volt d.c. circuit. I was trying to demagnetize an iron ring which had a primary winding wrapped around it. A double-pole reversing switch was in circuit. On pulling a long thin wire out of the acid I was able to reduce the current to a very small quantity. I had done this a number of times, and then suddenly on pulling it out once more I saw a yellow ball of fire about 0·1 in. diameter appear on the surface of the liquid and move slowly over it. Having travelled about 2 in. it suddenly disappeared completely with a terrific bang, and people including Prof. Clinton came running from several rooms away to see what had

* See W. J. Humphreys: "Physics of the Air" (Lippincott, Washington), p. 370.

happened. There I was, sitting innocently at my tank, and the liquid remained undisturbed. Naturally I made many attempts—about 800 altogether—to repeat the experiment, but was not successful. The only other case known to me where the same phenomenon has been "man-produced" is described in connection with the original researches of Gaston Planté in 1875, on breaking very heavy currents at the surface of liquids carried out.*

Mr. J. E. Taylor (*communicated*): In order to put the question of point discharge during thunderstorms to a crucial test I made up, a few days before the meeting, a simple appliance consisting of two parallel horizontal insulated brass plates fixed a centimetre apart. The upper plate was fitted with a central adjusting screw carrying a needle point, the gap between point and lower plate being adjustable from 1 cm. down to something over 0·5 mm. This appliance failed to show any discharge from the point up to the maximum voltage it was considered safe to apply. The result was that with 1 450 volts (d.c.) across the minimum gap no discharge took place. The galvanometer used was sensitive to at least 0·5 microampere. It will be seen that the stress was equivalent to well over 2 000 volts per millimetre, or 2 million volts per metre, whereas the highest stresses measured during thunderstorms, so far as I am aware, have been of the order of 10 000 volts per metre. The voltages were applied to the point in both directions.

Another way of exhibiting the failure of points to function as dischargers except at quite high electric stresses is to provide a Leyden jar with a needle point on its knob. If the jar be then charged from an influence machine to a high potential and removed, the needle will of course glow and discharge the jar more or less gradually down to a certain voltage, and then cease to function. There will, however, be a sufficient remanent charge to give a considerable shock should the observer venture to touch the knob and provide a circuit through his body. This means that the point will only discharge the jar until its minimum corona voltage is reached, when it will cease to function and at the same time it will cease to glow. Since the point functions in virtue of disruption of the air and we know of no case in which, under normal circumstances, such disruption is not accompanied by luminosity, the disappearance of luminosity is a pretty sure sign that the point has ceased to operate.

There seems to be an exaggerated idea extant as to the surface density of electric charges on projecting points. In the case of a point projecting from a plane well removed from other conductors, the maximum surface density is not so very much in excess of that on the plane. The mean surface density on a small rounded protuberance as compared with that on the plane on which it stands has been dealt with mathematically by J. J. Thomson.* From his treatment of this and other kindred cases it should be clear that even on a point the surface density is quite limited: this is evident from a consideration of the displacement field. By definition, a tube of displacement starting from a pointed projection represents the same total energy as a tube starting from the plane on which it stands. Since the outward divergence of a tube is limited by the constraining influence of neighbouring tubes from the plane, it is not possible for more than a certain proportion of the energy of a single tube to be concentrated close to the point. Thus it follows that the number of tubes which can converge towards the point, and therefore the surface density, must be quite limited.

The exaggerated view often taken of the discharging action of points is no new question. I have been aware of it for over 30 years, since the time of early wireless work when it was common to ascribe atmospheric disturbances to the "silent discharge" from antennae, under the influence of the atmospheric gradient or by induction from clouds.

The author appears to support the idea that points function in this manner during thunderstorms, even though he is somewhat guarded in his allusion. If, as seems to be the case, he relies largely on the St. Elmo's fire effect to substantiate his view, I fear he is trusting to a broken reed. The effect is relatively rare, and for this reason alone ought to be considered cautiously. Moreover, there is no proof of the assumption that it is a brush discharge; in fact, the low gradients of potential which seem to be the more normal condition in a thunderstorm do not bear this out. Even the gradients of 10 000 volts per metre sometimes attained are very far indeed from the value necessary to produce brush discharges. I consider the effect must, at present, be relegated to the list of unexplained phenomena, together with the slow flash, fireballs, etc. Local gradients may be argued, but there is no substantiation of their production to the necessary value except on the assumption that it *is* a brush discharge. If it could be shown that it is, then I would concede the point in regard to local gradients; but I have a strong leaning towards quite another interpretation of the effect. I consider it to be evidence of a strong concentration of ions from the atmosphere, attracted (generally) to some earthed projection. The author may not be aware that the phenomenon has sometimes appeared as a "blue flame running along the ground"; not even on a projection. It would seem probable that St. Elmo's fire is the visible sign of a much more common but less developed process: I mean that which evidences itself as the earth-to-air current during thunderstorms.

The idea that we are dealing with collections of charged ions or "space charges" produced in the atmosphere during thunderstorms has been in my mind for very many years, but it is only recently that I have stumbled across a curious confirmation. In taking readings of earth-to-air currents from collectors (commonly called "dischargers") on two poles only 100 ft. apart, I have found to my surprise that it is possible to get a current in one direction from one collector and in the reverse direction from the other at the same time. It is also possible to get a current from one to the other without an earth connection. This, in my view, completely disposes of the point-discharge theory.

Sir George Simpson has mentioned the slow lightning

* See Chap. 3 in the centenary volume of "Recherches sur l'Électricité," by G. Planté, issued in 1883.

† "Elements of the Mathematical Theory of Electricity and Magnetism," 3rd ed., Articles (99), (100).

which is observed in India and which it is not possible to explain on any theory yet advanced. Let me describe another form of lightning which has been observed in England on more than one occasion. It is the sustained flash to ground, persisting for perhaps the best part of a second. This flash has a reddish tinge and no particular brilliance as contrasted with the more common quick bluey-white flash. Apart from other features, its lack of brilliance precludes the explanation that persistence of vision is involved. Many years ago Sir Oliver Lodge informed me that he also had noted this flash and its peculiar persistence. Clearly there is a field of exploration of which, as yet, only the fringe has been touched. In my view that field lies in the recognition of space charges distributed in the atmosphere as the source of lightning effects; not in the setting-up of high gradients of potential between cloud and cloud or cloud and earth. The laboratory effects arising from surface charges have little connection with those which can arise from volume charges in a gaseous medium. Thus I consider the title of this paper a misnomer, since the longest spark within our knowledge is the lightning flash.

No doubt a great deal can be done in the laboratory to investigate space-charge effects if a suitable method be adopted. For instance, it should be possible to use the hydro-electric machine for the production of volume charges.

[The author's reply to this discussion will be found on page 492.]

NORTH-WESTERN CENTRE, AT MANCHESTER, 29TH NOVEMBER, 1938

Prof. E. W. Marchant: This is a very specialized subject, and I have been interested in it because it has thrown so much light on the nature of electrical discharges. We have been at work in Liverpool for about 8 or 9 years on an investigation of the behaviour of dielectrics under very high-frequency voltages. The account the author has given of the way in which a spark gradually feels its way through a dielectric throws a new light on the phenomena of breakdown, and explains why the apparent electric strength of air falls off with increasing frequency. The actual reduction in breakdown voltage between 5-mm. spheres at 1 million cycles per sec., as compared with 50 cycles per sec., is about 12 %, and this percentage reduction is independent of the electrode curvature until a stable brush discharge occurs.

One interesting phenomenon which is referred to by the author is the time taken for the space between the two electrodes to become ionized by the leader strokes. The space-charge theory which was developed by Townsend may be used to explain this effect, the gradual increase in the space charge during successive discharges being due to the smaller mobility of the positive ions, as compared with that of the negatively-charged electrons. The figure that is given by the author for the speed of formation of the leader stroke demonstrates this difference. The speed with positive ions is 6×10^6 cm. per sec., and with negatively charged particles 20×10^6 cm. per sec. This is a rough measure of the relative mobility of the positive and negative ions. A better explanation of the time-lag in discharges can be based on the phenomenon of leader-stroke formation, combined with the original space-charge theory.

The increase in our knowledge of the nature of high-voltage discharges during the last few years has been remarkable; a great deal of this development is due, first, to the study of lightning itself by Schonland; and secondly, to the high-voltage investigations that have been made in the laboratories controlled by the present author.

Prof. E. L. E. Wheatcroft: The long spark is part of the wide problem of the breakdown of gases, which has exercised physicists and engineers for at least all of this century. I am therefore much interested in speculating whether Dr. Allibone's new contribution throws any light on this larger problem.

Since Townsend's classical proof that the basic phenomenon involved in gaseous breakdown is ionization by electrons colliding with molecules of the gas, and his subsequent suggestion of ionization by positive ions, discussion has been concerned with this latter point of the secondary mechanism, with occasional doubts as to whether a secondary mechanism is necessary at all.

It is well known that uniform fields (e.g. spheres) and non-uniform fields (e.g. points) behave differently, in that in the former the first observed phenomenon is a spark, whereas in the latter the first observed phenomenon is corona. Townsend regarded the corona as a manifestation of breakdown of the same nature as glow. Peek regarded it as forming a kind of conducting zone which gradually increased the effective radius of the conductors until they became equivalent to spheres and so gave a spark. The author has shown us how this extension of the corona takes place, and has also shown, in agreement with Loeb's view, that the view taken by Peek was too simple. I have been much impressed by his demonstration of the "suppressed" spark, showing that the corona is fully formed in less than a microsecond.

The point of interest is whether this new evidence is to be regarded simply as showing the propagation of corona already formed, or whether it is also a contribution to the more difficult problem of the spark between spheres and gaseous breakdown in general. In considering the interpretation of the mechanism, we must not read more into the results than is there. The records are of luminosity, which is not of itself necessarily evidence of intense electric fields. Nor do I think they are evidence of recombination (which needs feeble electric fields) since it is fairly easy to show that only a small fraction of the electric energy will go into ionization and nearly all presumably into exciting the molecules of air.

Lastly, I want to set the author's work against certain other high-lights in the search for the explanations of breakdown, and of these I think the most interesting and important is, not the time taken in preparation for the spark, but the extreme suddenness of the voltage collapse which accompanies the main stroke or true spark, either with uniform or with non-uniform fields. Even on

oscillograms which show that this collapse occupies about 0·01 microsecond the change of slope of the voltage wave still appears as infinitely sharp. Has this new work anything to offer in explanation of that point? It does look as if there is a clue in the way in which the leader stroke feels its way slowly forward while there is still full voltage across the gap, and the instant the leaders join up there is immediate collapse. I hope the author will find some way of measuring the potential gradient along the leader as it is propagated.

Mr. G. J. Scoles: Since the work described in the paper was carried out an attempt has been made to determine which of the constituent gases in air are responsible for the phenomena observed. Experiments similar to those described in the paper have been carried out on nitrogen, oxygen, carbon dioxide, argon, and water vapour, and the results, expressed in general terms, are given below. All the experiments were carried out at pressures below atmospheric, partly because this enabled the apparatus employed by the author to be used, and partly to reduce the amount of gas required. All the following remarks refer to a pressure of 20 cm. of mercury.

(*a*) *Nitrogen.*—The general appearance of the spark is similar to that in air, but the leaders as shown by the rotating drum camera are usually much more marked and separated by as much as 120 microseconds. There are usually four or five of them, but very occasionally a spark occurs which is almost identical with that in air. The breakdown voltage is similar to that of air.

(*b*) *Oxygen.*—The colour of the spark is bluish, and the negative end is usually much more intense. The transition in intensity is sudden and occurs about halfway along the spark, while the two sections may be displaced laterally by as much as 0·5 cm. No leader mechanism has been detected, and the breakdown voltage is about twice that of nitrogen.

(*c*) *Carbon dioxide.*—This gives an intense greenish-white discharge, which the rotating drum camera shows to have a leader mechanism which is comparatively faint and totally unlike that of air. The breakdown voltage is about 35 % above that of air.

(*d*) *Argon.*—In this case the spark is mauve in colour and shows a tendency towards a glow or streamer discharge rather than a normal spark. This is confirmed by moving-film records which show a leader mechanism of the streamer type, followed by a main stroke of similar appearance. The breakdown voltage in this case is very low, being only of the order of 25 % that of air.

(*e*) *Water vapour.*—This was tested in a similar manner to the above substances, but as a pressure of 20 cm. of mercury was not obtainable only its effect when mixed with nitrogen will be described.

(*f*) *Mixtures of Nitrogen and Oxygen.*—A mixture of these gases in the ratio of 4 to 1 (by volume) gives sparks generally similar to those observed in air, but the leader mechanism is more like that of nitrogen than of ordinary air.

(*g*) *Mixtures of Nitrogen and Carbon dioxide.*—Mixtures of these two gases give a very intense spark, which the rotating drum camera shows to have leaders almost identical with those of nitrogen and to be followed by considerably more afterglow.

(*h*) *Argon-oxygen mixtures.*—In this case the addition of very small amounts of oxygen completely destroys the characteristics of the argon spark.

(*j*) *Nitrogen-water vapour mixtures.*—The addition of a small percentage of water vapour to nitrogen produces sparks very much like those observed in air, even when the rotating drum camera is employed. This does not apply to every spark, but on the whole a fairly close approximation to the air spark is obtained. From this it would appear that nitrogen and water vapour, and, to a lesser extent, oxygen are responsible for the characteristic spark of air, although not enough work has yet been done to enable definite conclusions to be drawn.

Mr. J. B. Hansell: There are one or two points regarding the formation of the main lightning stroke on which I should be glad if the author would give more information. He states that the leader stroke travels in a series of steps from a negatively charged cloud to earth, and that the leader stroke is followed by the main stroke travelling from the earth to the cloud along the path formed by the leader stroke. Fig. 7A shows this clearly. I should be glad if he would explain the mechanism of the formation of the main stroke, as, although it is produced by the same voltage as the leader stroke, it seems to be travelling in the opposite direction. Presumably, the current in the main stroke is considerably greater than that in the leader stroke; if so, I should be glad to know what is the source of this current, as the only direct path from the cloud is along the leader stroke.

Prof. Willis Jackson (*communicated*): I should be glad of the author's view as to the extent to which the mechanisms of breakdown dealt with in the paper might be operative in solids. In gases, the protection against breakdown is provided by the conversion into excitation energy within the electronic system of the gas molecules of much of the energy of the accelerated free electrons and positive ions. In liquids and solids, the much closer packing of the atoms and molecules introduces additional barriers to ionization since the mean free path is much smaller, the accelerated electrons are liable to lose much of their energy in exciting inter-atomic vibrations, and the positive ions are immobile and therefore cannot contribute directly to maintaining and augmenting the supply of free electrons. For voltages of very short duration, however, the ions in gases are for all practical purposes also immobile, and I cannot see, therefore, that there is likely to be any essential difference in principle between the behaviour of solids and gases.

I agree with the author in his inability to accept Strigel's view that, in the case of the positive point/plane cathode, electrons have first to be released from the cathode and travel to the anode before the positive leader begins to extend towards the cathode. It is possible to visualize circumstances where this would apply, but in the presumably more usual case of uniform initial electron distribution, the electrons near the anode, i.e. in the region of greatest potential gradient, will surely be the ones to initiate the discharge.

Mr. J. M. Meek (U.S.A.) (*communicated*): The study of the leader/main stroke sequence for the electric spark has made such rapid progress since the original date of submission of the present paper that it is difficult to make any criticism without an assumption of knowledge

which was not then available to the author. However, I should like to make a few comments on statements which I consider to be in error.

On page 515 the author refers to the curve shown in Fig. 1, and states that α changes from 8 to 80 for a change of X/p from 40 to 60. From measurements on this curve, I find the change in α to be 4·8 to 60 ($X/p = 40$, $\alpha = 760 \times 10^{-2\cdot 2} = 4\cdot 8$; $X/p = 60$, $\alpha = 760 \times 10^{-1\cdot 1} = 60$). I mention this difference in our estimated values to emphasize the necessity of accurate determination of α if a reliable estimate is to be made of the number of electrons produced by a single electron avalanche. The number of electrons so produced per centimetre, for $\alpha = 4\cdot 8$, is $e^{4\cdot 8} = 120$, while for $\alpha = 8$ the number is $e^8 = 3\,000$. Again, the number of ions produced in air at atmospheric pressure for $X/p = 40$ is given on page 516 as 10^4, corresponding to $\alpha = 9$. The curve given in Fig. 1 is for pure N_2, and the most recent values that I can find for air are those given by Sanders,* in which $\alpha = 12\cdot 7$ for $X/p = 40$, and the number of ions produced by a single electron avalanche is $e^{12\cdot 7} = 3\cdot 2 \times 10^5$ per cm.

An error seems to have been made by the author on page 521 in the determination of the number of electrons produced by a single electron avalanche which traverses the whole length of the chamber. If one takes the range of values 0·05–0·1 for α as given by the author, the number of electrons produced is $e^{3\cdot 8} - e^{7\cdot 6}$ (electrode spacing = 76 cm.), i.e. 45–2 000, and not 100–130.

Clearly, an accurate determination of the number of ions formed by collision processes is dependent on accurate determination of α, and it is thus interesting to observe that all the experiments to determine α until those performed by Kruithof and Penning,† and Bowles,‡ have been carried out in gases contaminated by mercury vapour. This contamination was unavoidably present owing to the use of mercury pumps and pressure gauges and was of the order of 10^{-3} mm. pressure. Bowles found that while the contamination had little effect for high pressures and low values of X/p, yet it increased the value of α/p by 17 % for low pressure and high X/p (about 500). Thus considerable discretion must be exercised when one refers to the values of α as given by the earlier experiments.

In reference to the negative branched discharge shown in Fig. 3, the author states "clearly spark appearance cannot be taken as a guide to the polarity of the electrode from which the discharge develops." While I agree that the appearance of the discharge as recorded by a still camera is not sufficient evidence to enable one to infer the polarity of the electrodes, yet I feel that the difference between the methods of progagation of the negative and positive leader strokes as revealed by the rotating camera is quite a definite indication of electrode polarity. Also, on page 518, the author expresses the opinion that the appearance of the leader/main stroke combination in the lightning discharge cannot now be regarded as a criterion for deducing the cloud polarity. As yet I feel this statement to be rather premature, particularly in view of Schonland's assertion* that the lightning discharges which he has photographed emanate from negatively charged clouds, so that the discharge from a positive cloud may not yet have been photographed, and may exhibit slightly different characteristics to that from the negative cloud. However, Schonland bases his assertion on oscillographic measurements of field change (some of which are synchronized with photographs), and the validity of such measurements may be questioned owing to the fact that inter-cloud discharges frequently accompany discharges to earth, so that the oscillograms do not give a true record of the change in electric field due to the discharge to earth only. But, since both positive and negative field-changes are known to exist,† it will be interesting to see a rotating-camera photograph of a lightning discharge which may be believed to be positive from field-change measurements.

* *Physical Review*, 1933, vol. 44, p. 1020.
† *Physica*, 1936, vol. 3, p. 515, and 1937, vol. 4, p. 430.
‡ *Physical Review*, 1938, vol. 53, p. 293.

It is evident that considerable research remains to be done by means of the rotating camera, particularly in conjunction with the cathode-ray oscillograph. Only the discharge in air at atmospheric and sub-atmospheric pressures has been studied to any extent, and one may expect considerable changes in the development of the discharge for different gases. Additional work might well be performed with different electrode shapes and arrangements. Also, all the rotating-camera records that have been described refer to discharges produced by impulse voltages, and further study might be continued with a.c. and d.c. discharges, when a considerable time is available for an appreciable space-charge to be built up in the gap before the leader stroke develops.

Dr. J. L. Miller (*communicated*) :‡ It is interesting to note that the upward leader stroke, the eventual discovery of which was predicted in the paper (page 518), has very recently been found in nature by McEachron§ when investigating lightning in relation to the Empire State Building. Further, it appears that such upward leaders are followed by what McEachron calls a "continuing stroke," which persists for a time of nearly a second and during the existence of which there appear to be superimposed current peaks, themselves often preceded by downward leaders. While the latter part of the phenomenon may be peculiar to characteristics of the Empire State Building, nevertheless one feels that Dr. Allibone has been given more than he bargained for, and I should very much like to have his comments.

On page 518 the author explains the "filtration" effect of points like transmission towers and uses a laboratory experiment to show the selective action of a point in attracting the negative discharges. It is with interest, therefore, that one reads in McEachron's paper "discharges to the building with positive cloud occur very rarely, if ever."

[The author's reply to this discussion will be found on page 492.]

* *Proceedings of the Royal Society*, A, 1938, vol. 164, p. 132.
† J. C. Jenson: *Journal of the Franklin Institute*, 1933, vol. 216, p. 707; G. C. Simpson and F. J. Scrase: *Proceedings of the Royal Society*, A, 1937, vol. 161, p. 309.
‡ Dr. Miller also contributed verbally to the discussion. The substance of his remarks will be found in the report of the London discussion (see page 486).
§ *Electrical Engineering*, 1938, vol. 58, p. 493.

SCOTTISH CENTRE, AT EDINBURGH, 28TH FEBRUARY, 1939

Prof. M. G. Say: Man is a long way behind Nature in producing long sparks, and it is an achievement to have got a 400-kV spark into a lecture room. I think the author must be one of the first to have done so. I congratulate him on the smallness of his impulse generator; it seems a beautifully economical design.

Comparing the natural phenomenon with the experimental, a cloud always appears to be in effect a point, and the earth a plane, or pointed plane. Now, the leader stroke is always downwards (i.e. to earth), although it may be met by upward leader or attempted leader strokes, depending on polarity. It is immediately followed by an "upward" main stroke. Why should the main current be upwards? The main stroke is unidirectional, not oscillatory, and the term "upward" must refer to something other than the current direction. Perhaps a more careful definition of terms is necessary here. In any case, some details of the mechanism of the main stroke would be of great interest.

Mr. G. F. Moore: In connection with lightning, I wonder whether it is safer to have a number of points on the surface than to leave a chimney without any further projection than that of itself.

Mr. B. K. Pilkington: I am personally interested in the construction of underground buildings. In these would it be an advantage to place lightning conductors at the side of the building instead of over the top?

THE AUTHOR'S REPLY TO THE DISCUSSIONS AT LONDON, MANCHESTER, AND EDINBURGH

Dr. T. E. Allibone (*in reply*): The paper was originally written as a monogram for the Coopers Hill War Memorial Competition of The Institution, the subject being classed under the heading of "electrical science," and was not written with a view to its being read before The Institution: the very lively discussion which the paper has invoked is therefore most gratifying and indicates that the subject matter is of greater general interest than I had supposed. As Dr. Miller remarks, the object of the work was to learn more about the physics of spark formation and to investigate what bearing this could have on the problem of understanding the lightning flash; speakers have made valuable contributions to the discussion on both these aspects and I will segregate my reply into these two arbitrary divisions.

First I should explain that the paper was written in July–August, 1937, and since then two more papers have been published giving much more data on the spark discharge; detailed answers to many of the speakers' questions may be found in these papers,* and I will not therefore reiterate unduly. The rotating-drum camera is not at all like the Boys' rotating camera, the novel feature of which was the incorporation of two lenses at opposite ends of a diameter. Such a camera is essential to determine the speed of formation of a lightning flash, since the point of origin of the flash is not known. For spark photography the only requisite is a film moving in the focal plane of a lens at a known speed at right angles to the path of the spark. The simple spark, as shown in Figure 7B, consists of a leader stroke propagated relatively slowly, followed by a main stroke propagated at much higher speed. It can be shown that the speed of the main stroke is beyond the resolution of the camera, so that the speed of the leader stroke is easily deduced from the lateral displacement of the leader with respect to the main stroke. The camera consisted merely of a stationary lens, and a drum $10\frac{1}{2}$ in. diameter rotating at 3 000 revs./min., and a film was attached rigidly to the outer periphery of the drum; other details are given on page 516 (vol. 82).

Multiple spark discharges similar to the multiple lightning flash have now been produced in the laboratory under conditions very similar to those probably existing in the cloud, so I would refer Prof. Thornton to the spark photographs and current and voltage oscillograms opposite page 113 of the Royal Society paper. The spectrum of the impulse spark is the spark spectrum of air; spectrum lines corresponding to the material of the electrodes exist at the extreme ends of the spark, but there is insufficient time for the electrode vapours to cross the gap before the spark is extinguished: the spectrum of the continuous discharge shows the presence of metallic vapour over the whole inter-electrode gap. I am afraid I cannot answer his questions about the spin of the spark.

The development of the Lichtenberg figure is, in my estimation, similar to that of the leader stroke. Lichtenberg figures are not usually taken with the main stroke on them, as this would blur the initial or leader stroke figure. I do not follow Dr. Rayner's distinction between the fields causing the positive and negative figures: surely the positive and negative fields are not only on the solid surface but extend into the space around. The difference in form of the figures is not easily explained; why are the negative figures fan-shaped, the tufts being relatively straight, whereas the positive figures look like tree branches? This question has never received a satisfactory answer. The difference in size can, however, be explained by the same argument advanced to explain why the negative sparkover voltage is higher than the positive voltage for a point/plane gap. This is described on page 518, col. 2, of the paper (vol. 82).

In reply to Dr. Wilson, I do not think the presence of the glass photographic plate modifies the discharge between point and plane as shown in Fig. 5 to any great extent except that the sparkover voltage is lowered. I think the Lichtenberg figures are true records of the light emitted when the discharge occurs, for it is usually possible to see discharges which are intense enough to be registered photographically as Lichtenberg figures. The disadvantage of using the rotating camera, as Dr. Wilson suggests, to record directly the progress of Lichtenberg figures is that the available speeds would be totally inadequate to detect progressive development. A better scheme which I have in mind is to photograph the long spark with the rotating camera as the spark develops over an insulating surface.

* T. E. Allibone and J. M. Meek: *Proceedings of the Royal Society*, A, 1938, vol. 166, p. 97, and vol. 169, p. 246.

I am indebted to Dr. Whitehead for his interesting summary of the phenomena of breakdown between spheres as the spacing between them is increased. Given adequate resolving power, it would be instructive to follow the course of breakdown with the rotating camera as the spacing is increased from the uniform field condition to the conditions existing when the spheres are many diameters apart—the point/point condition.

I have had similar experiences to those quoted by Dr. Miller with respect to sphere-gap sparkover, but I have ascribed them to inadequate intensity of ionization. The details are given in Messrs. Edwards and Smee's paper:* radium and ultra-violet light render the action of small gaps consistent, but do not do so for large gaps. I think the erratic behaviour must indicate statistical and not formative time-lag, because Prof. Thornton's parallel-plate gap set for the same voltage (and probably having the same formative time-lag) is far more consistent than a sphere gap.

For measurement of time-lag, the cathode-ray oscillograph was connected to a resistance potentiometer in parallel with the discharge gap, so that times to breakdown were accurately recorded. In the later work reported in the Royal Society paper very high series resistances were sometimes employed between the impulse generator and the discharge gap, and in such cases the resistance of a divider placed across the gap would have had to be so large that the oscillogram would not have been a true record of the voltage of the gap. On the other hand, as the use of a capacitance voltage divider was known to affect the discharge process, an antenna had to be used to pick up a potential proportional to the field. It is recognized that this system cannot be very accurate as regards voltage measurements, but there is little likelihood of errors in recording the time intervals between discontinuities on the voltage waves, and these were the important measurements to correlate with the rotating camera records.

In reply to Mr. E. O. Taylor, one of the oscillograms shown as a slide was recorded at the electrode when a high value of series resistance was in circuit, and the stepped nature of the discharge is doubtless due to steps in the rise of voltage at the point, as indicated on page 519 (vol. 82). This is discussed in the Royal Society paper (vol. 169). The currents were measured oscillographically as given in the earlier Royal Society paper. Mr. Taylor's Fig. A is unfortunately a klydonogram of a *positive* discharge with a *negative* figure at the centre: the existence of the negative figure indicates either that the impulse was oscillatory, or that it was chopped by a parallel gap, or that it was an impulse of very short wave-tail.

The horizontal striations mentioned by Mr. O'Dea may be ionized pockets as he suggests or may be due to incandescence of dust particles. They are characteristic of all spark and lightning photographs taken with a rotating camera, as mentioned by Prof. MacGregor-Morris.

The later work given in the two Royal Society papers answers many of the points raised by Prof. Wheatcroft. When the leader stroke reaches the opposite electrode the full voltage is then across a semi-conducting filament instead of across a good insulator, and it is true that voltage oscillograms show the subsequent collapse of the voltage to be very rapid indeed. The camera records also show that the main stroke bridges a gap 1 metre long in less than ½ microsecond, which supports the oscillographic observation.

Mr. Hansell and Prof. Say raise a very real difficulty in the understanding of the leader/main stroke combination. I avoided an explanation in the paper as it cannot be given in a few words. It is true that the main stroke develops in the opposite direction to the leader stroke: what develops or progresses is the "head of the disturbance," not the charge: the charge in the main stroke travels in the *same* direction as in the leader stroke because, of course, the polarity of the electrodes (or cloud) is not changed. Take the case where the upper electrode is positive. The downward movement of the leader is the downward movement of the *tip of an ionized column* extending from the upper electrode; the moving particles are electrons ahead of the tip moving into the tip and then up the conducting channel to the top electrode. At the moment when the leader stroke just reaches the ground electrode the path from ground to upper electrode is a semi-conducting path, the conductivity of which is *greatest* at the ground end where the track is freshest; and is *lowest* at the upper end where the track is some microseconds old and where recombination of positive ions and electrons has been proceeding all the time the leader has been travelling to ground. The whole applied voltage which impelled the leader stroke forward is still maintained across this semi-conducting path between electrodes. The result is that, owing to further ionization by collision in this channel, the conductivity rises very rapidly and is greatest first where the density of ions is highest, viz. at the ground electrode. The potential drop across an element of channel here therefore falls and imposes an increased potential across the neighbouring element of channel further from the ground electrode: the conductivity here rises, and in turn imposes an increased potential across the next higher element, and so on. Thus the head of the second disturbance, the region of *very* intense ionization, travels progressively upwards from ground to the top electrode: this is the development *upwards of the main stroke.* A similar explanation may be given, with suitable adjustments, for the case where the upper electrode is negatively charged. The full picture of the development of the leader and the main stroke must of course include the displacement currents in the dielectric surrounding the ionized channels.

I am hoping to extend the present researches to the breakdown of transparent solid dielectrics: I agree with Prof. Jackson that probably the same mechanism holds, and already we have evidence of leader strokes in liquid dielectrics.

Mr. Scoles has given a précis of work we are now undertaking on the discharge in different gases, and although some gases appear to develop without a leader stroke mechanism it is too early to state how development occurs: it may be a matter of adjustment of conditions of the experiment.

I am indebted to Mr. Meek for his contribution. Actually Fig. 1 is a composite curve based on many curves by Posin (see reference) which do not fit together very

* *Journal I.E.E.*, 1938, vol. 82, p. 655.

well. I do not think that these errors are important at the present state of our knowledge, because, as Mr. Meek points out, the values may be affected by the presence of mercury vapour, and Mr. Scoles has already shown that the leader stroke develops differently in the gases which compose air, so a knowledge of α for pure N_2 is not very helpful.

With regard to the appearance of the positive and negative discharge, I am not prepared yet to agree that the difference in method of propagation of the negative and positive leader stroke affords indication of electrode polarity: recent work shows that the method of propagation can be altered by alteration of circuit constants as well as by change of polarity. I can therefore only reiterate that, in the absence of oscillographic or other proof of lightning-stroke polarity, the appearance of a leader/main stroke combination gives no clue at present to polarity.

I will now deal with the discussion which related to the lightning flash. I was very interested in Sir G. Simpson's reference to the exceptional types of lightning, but I cannot offer explanations of value. I have one suggestion to make with regard to ball lightning, based on a paper by Th. Neugebauer entitled "The Problem of Ball Lightning" (*Zeïtschrift für Physik*, vol. 106, p. 474). The explanation given by Neugebauer involves a very high density of ions. Now in all of the spark photographs where a positive and a negative leader stroke meet, the meeting point is characterized by a much greater light intensity than elsewhere in the spark. In a few records [see Fig. 8(*a*) of the Royal Society paper (vol. 616)] this meeting point is a sphere of gas discharge 7 mm. diameter, the luminosity of which persists a very long time (something similar may be seen at the junction of the leaders in Fig. 8A of the present paper). It is suggested tentatively that this is a region of high ionic density and fulfils the conditions of non-degeneracy set up by Neugebauer. On this basis, ball lightning probably arises at the junction of the downward lightning leader stroke and a short upward directed leader stroke, the combination giving the required ion density for Neugebauer's equation.

In reply to Prof. Thornton, the difference in time-lag is due to the difference in length of the lightning flash and the spark discharge, since time-lag equals distance travelled divided by velocity, and $v \simeq 10^7$ cm./sec. Prof. Thornton quotes a case of St. Elmo's fire on the lamp posts at Dieppe; these things are fairly common in spite of what Mr. J. E. Taylor says. I would refer Mr. Taylor, as Prof. Goodlet did, to Prof. Schonland's many papers on lightning, and this remark applies also to Dr. Wilson's question about diameter of the lightning flash.

With regard to Dr. Miller's comments, multiple strokes have now been produced in the laboratory (see the Royal Society paper, vol. 166) and recorded on transmission lines by Berger, and by McEachron.* The present indications are that current flows in the channel for quite a long time (more than 100 microseconds), which confirms Appleton and Chapman's observations.† McEachron also shows that a weak current can still be flowing at the moment when one of the later components of a multiple stroke develops. It is of course difficult to formulate ideas as to what is happening in the cloud when multiple strokes are formed. I think the stepping of the initial leader may be due to the rate of rise of voltage as well as to some basic phenomenon in the formation of the discharge in air. For the second and subsequent leaders which are not stepped but proceed continuously to ground the conditions in the air are very different: the air is still partially ionized and therefore a much lower voltage will cause flashover—this is shown for a spark in Fig. 18 of the Royal Society paper (vol. 166). Arguing from this, subsequent lightning flashes probably occur when an intercloud flash taps a relatively small volume of charge. I was most impressed with Mr. McEachron's recent paper quoted by Dr. Miller, especially with the evidence of an upward leader to the first stroke followed by downward leaders to subsequent strokes. The upward leader is to be expected from a study of the spark discharge between pointed electrodes, but laboratory experiment cannot forecast whether any subsequent leader passing over the weakly ionized trail left by the first main stroke will start at the cloud or at the earthed electrode: apparently, in general it occurs from the cloud downwards.

In connection with Dr. Rayner's observation that the attractive power of earthed points to negative sparks was demonstrated at the National Physical Laboratory, I agree that the observation is not new; a difference in the behaviour of a positive and negative spark was noted by Peek years ago, but when the Lewis and Foust and the Grünewald statistics of polarity of lightning flashes to transmission towers were published I think they were generally accepted as indicative of the preponderance of negatively charged clouds; in my view that deduction is fallacious.

In reply to Mr. Moore and Mr. Pilkington, there is no doubt that all high buildings ought to be protected by a good lightning conductor. I do not appreciate the form of construction of the underground buildings, but the lightning conductor ought to be well earthed and amply cover the exposed area of the building.

* *Transactions of the American I.E.E.*, 1938, vol. 57, p. 510.
† *Proceedings of the Royal Society*, A, 1937, vol. 158, p. 1.

THE DEVELOPMENT OF A SMALL VARIABLE AIR CONDENSER COMPENSATED FOR RAPID CHANGES OF TEMPERATURE *

By H. A. THOMAS, D.Sc., Member.

(*Paper first received* 10*th September, and in revised form* 16*th November*, 1938.)

SUMMARY

Various methods are discussed of obtaining compensation for temperature variation in air-dielectric condensers of small capacitance value suitable for radio-frequency purposes. It is found that appreciable air-gap spacing is necessary for high stability and a high degree of compensation. It is concluded that a parallel-plate system affords the most satisfactory means of achieving high stability.

A description is given of a variable air condenser of small dimensions and weight which has a capacitance range of 17–68 $\mu\mu$F and a high degree of temperature compensation; the overall dimensions are $2\frac{1}{2}$ in. × 2 in. and the weight is $5\frac{3}{4}$ oz. The temperature-coefficient of capacitance can be adjusted to have any value within the range zero to −120 parts in 1 million per degree Centigrade, and the compensation is effective at any rate of temperature variation not exceeding 5 deg. C. per minute.

When the system is used in association with a ceramic-former coil, its frequency coefficient can be made as low as 1 part in 1 million per deg. C., and the frequency stability over long periods with large temperature-change is of the order of 10 parts in 1 million.

(1) INTRODUCTION

In a recent paper† a description was given of a variable air condenser with adjustable compensation for temperature. The knowledge derived from a detailed study of the factors causing variation of capacitance‡ was used to make the compensation effective for comparatively rapid changes of temperature. The electrical performance was much superior to that obtained with previously-constructed temperature-compensated condensers. This condenser was, in fact, quite satisfactory for many purposes, but to meet other requirements it was decided to apply these same fundamental principles of compensation to a condenser of much smaller dimensions and reduced weight. With this object in view a study has been made of various means of constructing condensers of small bulk and high electrical stability.

(2) METHODS OF OBTAINING TEMPERATURE COMPENSATION IN SMALL AIR CONDENSERS

Methods alternative to a parallel-plate construction have been explored. Since the use of a very small air-gap enables an appreciable capacitance to be obtained with small dimensions, experiments have been made on two special condensers embodying unusually small air-gaps. The first condenser consisted of an eccentrically-mounted rotor of semi-cylindrical cross-section and an earthed cylindrical stator. The rotor was made of invar and was supported by spring-loaded balls, and the stator was made of brass. By turning the rotor through 180° the capacitance could be varied from 17 to 57 $\mu\mu$F. The performance of this condenser with temperature variation was cyclic, and the temperature-coefficients of capacitance at the maximum and minimum settings were − 350 and − 150 parts in 1 million per deg. C. respectively. A large negative value of the temperature-coefficient of capacitance was anticipated owing to the large fractional change of air-gap produced by the differential expansion of the rotor and stator.

This condenser is suitable as a trimmer with a large negative capacitance coefficient, but it is not satisfactory as a variable air condenser having an adjustable temperature-coefficient of capacitance for the following three reasons:—

(*a*) The temperature-coefficient of capacitance is large and negative, and its value cannot be varied over a wide range.

(*b*) Variation of the coefficient is accompanied necessarily by change of capacitance.

(*c*) Slight variations of the expansion coefficients of either the rotor or the stator cause considerable change of temperature-coefficient of capacitance, and accurate reproduction of the assembly is consequently difficult.

In the second experimental condenser an attempt was made to reduce the dependence of the coefficient of capacitance on the capacitance setting by making use of a method of compensation adopted previously.† This condenser consisted of a semi-cylindrical rotor mounted inside a hollow cylinder, steel balls being used as bearings; both rotor and stator were made of invar. Variation of capacitance was arranged by rotation of the earthed stator system with respect to the fixed rotor. The capacitance range was 20–66 $\mu\mu$F.

The temperature-compensating arrangement consisted of a brass tube fixed to the stator in such a manner that increase of temperature gave rise to expansion of this tube, and consequently to a relative axial movement between the stator and rotor. The dimensions were selected to give theoretically a negative coefficient of about 20 parts in 1 million per deg. C.

Tests on this condenser showed that the performance with temperature-change was cyclic but that the coefficient of capacitance had a large negative value. At

* Official communication from the National Physical Laboratory.

† H. A. THOMAS: "A Variable Air Condenser with Adjustable Compensation for Temperature," *Journal I.E.E.*, 1937, vol. 81, p. 277.

‡ H. A. THOMAS: "The Electrical Stability of Condensers," *ibid.*, 1936, vol. 79, p. 297.

† *Journal I.E.E.*, 1937, vol. 81, p. 277.

the maximum and half capacitance settings the coefficients were — 125 and — 450 parts in 1 million per deg. C. respectively. This behaviour is due partly to the very small air-gap and partly to the difficulty of maintaining concentricity of the cylinders with angular rotation. Moreover, if the expansion coefficients of the rotor and stator are different by only 1 part in 1 million the temperature-coefficient of capacitance may be changed by 120 parts in 1 million per deg. C. Since the expansion coefficient of commercial invar may lie between — 2 and + 2 parts in 1 million per deg. C. it is apparent that the adoption of very small air-gaps may give rise to variations in capacitance coefficient between identically-constructed condensers of the order of 500 parts in 1 million per deg. C.

Tests on these two condensers have shown that the use of small air-gaps may give rise to considerable uncertainty of behaviour, owing to the fact that the expansion coefficients of the materials used are not known to a sufficient degree of accuracy, and normal methods of construction are insufficiently precise. If high electrical stability is desired, multiple air-gaps with appreciable spacing should be employed; there seems, in fact, to be no satisfactory alternative to a parallel-plate assembly.

(3) DESCRIPTION OF SMALL TEMPERATURE-COMPENSATED CONDENSER

The constructional details of this condenser are illustrated in Fig. 1. Since the temperature-compensating system functions by using the difference in expansion between different metals, it is necessary to construct both the stator and rotor assemblies of the same metal and to use another metal for the compensating assembly. Brass and iron are convenient metals for this purpose and, since a lesser degree of compensation is required if the stator and rotor plates are made of the material having the smallest expansion coefficient, these assemblies are made of iron. The stator and rotor plates are conventional in form and are soldered respectively to the three steel supporting rods (5) and the steel spindle. This spindle is located and insulated respectively by two steel balls and " Calit " plugs I, and is rotated by the insulated bracket (9) and drive shaft. Slight pressure is maintained on the locating balls by the spring plate (6).

Compensation for temperature-change is effected by three brass rods (4), which on expansion increase the air-gap on one side of the rotor plates and reduce the gap on the other side. The length of these rods is selected so that, when the air-gap on one side of the rotor plates is twice that on the other side, the differential expansion between the brass rods and steel shaft is such as to produce exact compensation for temperature. Adjustment of the temperature-coefficient of capacitance it provided by a screw (1); rotation of this screw produces an axial movement of the rotor assembly with respect to the stator; the adjusting screw may be locked in any position by means of the nut (2). The spring plate (6) serves the purpose of maintaining location of the rotor over an axial movement corresponding to the difference between the two extreme positions defined by contact between the rotor and stator assemblies. The negative temperature-coefficient of capacitance may be varied by adjustment of the rotor position relative to the stator plates.

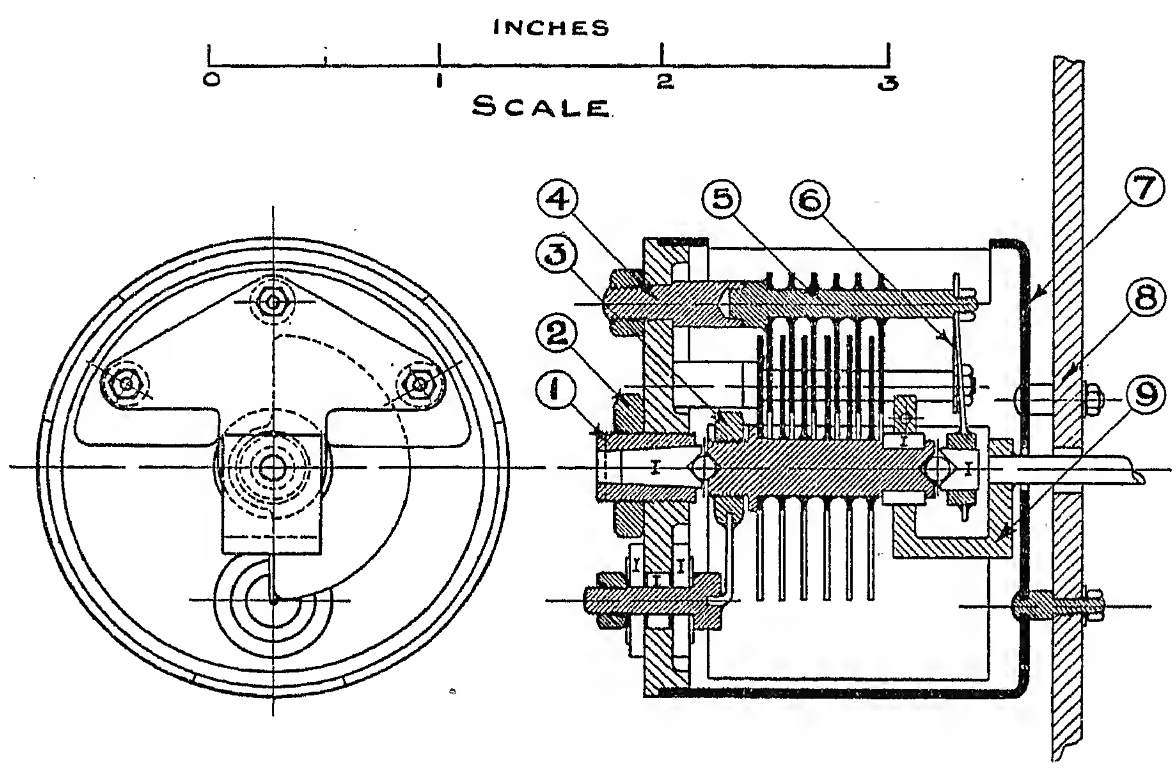

Fig. 1.—Assembly of temperature-compensated condenser.

In this particular condenser the rotor and stator vanes were made of iron and the compensating rods of brass, but it is not essential to adhere to this particular disposition. The stator and rotor assemblies can be made of brass or aluminium, in which case the compensating rods could be made of iron; if this modification is adopted a negative temperature-coefficient of capacitance is obtained when the rotor is moved as far as possible to the left (see Fig. 1), but the magnitude of the coefficient is less than that given with stator and rotor plates made of iron.

The stator is connected to the earthed frame and is

enclosed in an aluminium case (7) provided with large ventilating holes; copper gauze is used to effect complete electrical screening. Connection to the rotor is provided by the sliding nut (3). The complete assembly is mounted behind the panel (8) by three pillars fixed to the aluminium case. All the metal parts are chromium-plated to give a smooth surface and prevent rusting. The overall length and diameter of the condenser are $2\frac{1}{2}$ in. and 2 in. respectively; the weight of the complete assembly is $5\frac{3}{4}$ oz. (160 g.). The capacitance range is 17–68 $\mu\mu$F for a 180° rotation of the driving spindle, and the power factor is about 5×10^{-4} at a frequency of 5 Mc./sec.

(4) DEGREE OF COMPENSATION OBTAINED

(a) Heating Tests on Condenser

The condenser was set up in the testing chamber used previously and tests were made to ascertain the possible

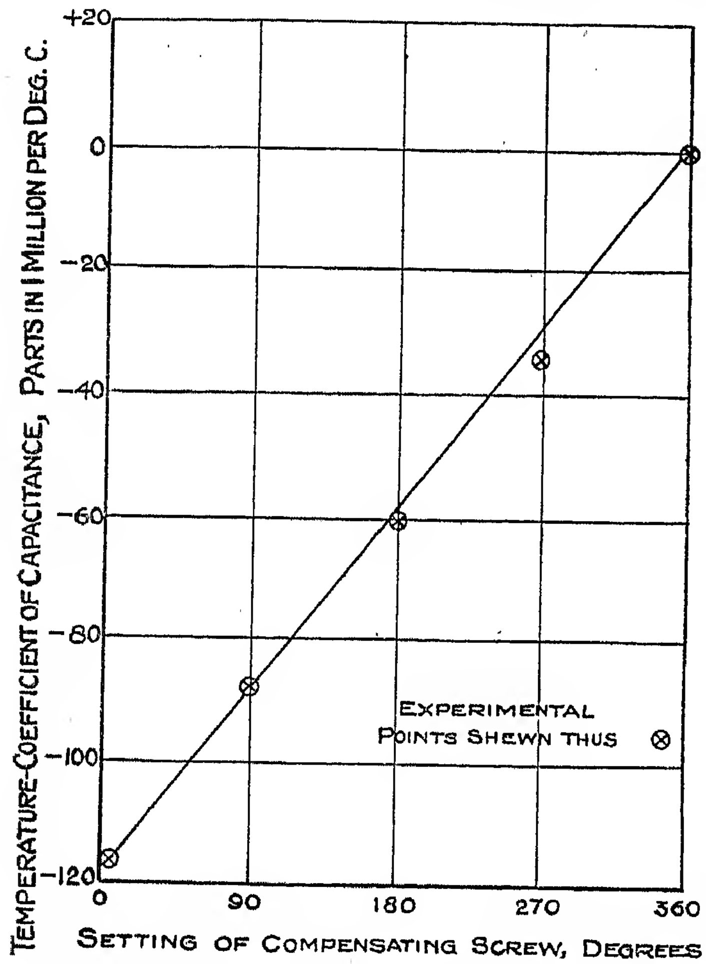

Fig. 2.—Relationship between capacitance coefficient and setting of compensating screw.

range of adjustment of the temperature-coefficient of capacitance. It was found that any desired temperature-coefficient between zero and −120 parts in a million per deg. C. could be obtained by adjustment of the compensating screw. For a capacitance setting of 36 $\mu\mu$F the relationship between the temperature-coefficient of capacitance and the setting of this screw is shown in Fig. 2.

The compensating screw was adjusted to give a negative temperature-coefficient of about 18 parts in 1 million per deg. C. at a capacitance setting of 68 $\mu\mu$F, and tests were undertaken to ascertain the electrical performance under varying conditions of temperature-change. The nature of the capacitance variation can best be illustrated by reference to Fig. 3, which shows the effect of heating the condenser at a rate of about 5 deg. C. per minute. It is seen that at this rate of heating the capacitance variation followed very nearly the temperature-change,

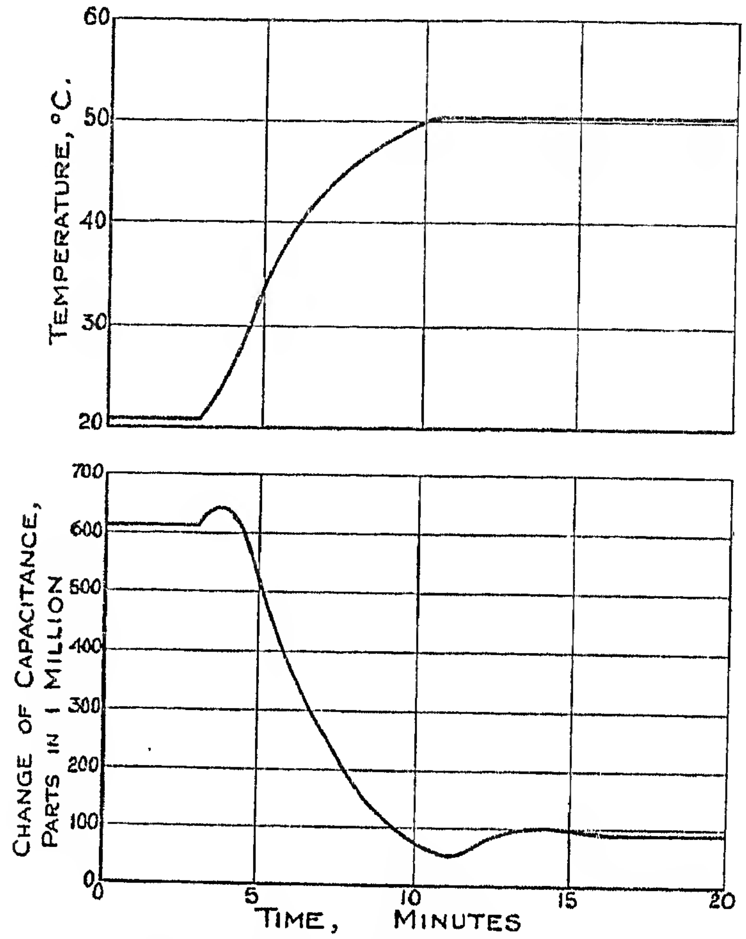

Fig. 3.—Example of rapid heating.

the temperature-coefficient of capacitance being −18 parts in 1 million per deg. C. At slower rates of heating no transient effects of any kind were observed.

At the same setting of the compensating screw the temperature-coefficient of capacitance was measured at various capacitance settings; the results are shown in Table 1.

Table 1

Variation of Temperature-coefficient of Capacitance with Capacitance Setting

Capacitance, $\mu\mu$F	Coefficient of capacitance, parts in 1 million per deg. C.
25	− 97
36	− 59
55	− 30
68	− 18

In this Table the temperature coefficients have been deduced from the observed frequency variations on the assumption that the valve and circuit capacitance of 17 $\mu\mu$F is constant.

The results demonstrate that considerable variation of temperature coefficient occurs with setting of the rotor; this is probably due to the fact that the rotor and stator vanes are not exactly parallel. This variation of co-

efficient with capacitance setting is obviously not important when frequency stabilization is required over only a narrow frequency band. For applications in which a high degree of stabilization is required over a wide frequency band it is probable that this variation of capacitance coefficient with rotor setting could be reduced appreciably by improvement of the assembly with respect to the parallelism of the rotor and stator plates. It must, however, be pointed out that, even if the temperature coefficient were made independent of the setting, the other capacitances of the circuit (i.e. valve, coil, and lead capacitances) would necessarily limit the range of frequency over which a high degree of stabilization could be expected.

(b) Heating Tests on a Temperature-compensated Oscillator

The condenser was now associated with a ceramic-former coil of inductance $11 \cdot 4\,\mu$H to form a closed circuit in which oscillation was maintained by a dynatron valve system; the mechanical and electrical properties of the coil have been described in a previous paper.* The capacitance was adjusted so that the oscillation frequency was $6 \cdot 5$ Mc./sec. At this frequency the temperature-coefficient of inductance of the coil was $+ 28$ parts in 1 million per deg. C.

The complete oscillator was put into an oven and the compensating screw was adjusted until the frequency coefficient of the oscillation circuit was very small. The temperature was increased regularly from 20° to 50° C. in 12 hours and was then decreased to 20° C. during the next 12 hours. This thermal cycle was repeated for 7 consecutive days, during which observation was made frequently of the oscillation frequency. It was noticed that the frequency-changes followed exactly the temperature variations and that the frequency coefficient of the oscillator was about $+ 1 \cdot 5$ parts in 1 million per deg. C. The total drift in frequency over this period of 1 week was less than 10 parts in 1 million.

* H. A. THOMAS: "The Dependence on Frequency of the Temperature-Coefficient of Inductance of Coils," *Journal I.E.E.*, 1939, vol. 84, p. 101.

(5) CONCLUSION

Observation of the performance of this condenser shows that a very high degree of frequency stabilization can be attained with a ceramic-former coil in association with a condenser of special construction. The condenser necessary for this purpose can be made quite small in both dimensions and weight if the required capacitance range is not very large. Such a condenser can be designed to respond to variations of temperature as rapid as 5 deg. C. per minute, this rate of temperature-change being greater than that obtained in nearly all practical applications.

(6) ACKNOWLEDGMENTS

The work described in this paper was carried out as part of the programme of the Radio Research Board and is published by permission of the Department of Scientific and Industrial Research. The author is indebted to Mr. R. G. Chalmers for assistance in the experimental work, and to Mr. F. G. Murfitt for the construction of the condenser.

THE QUADRATURE TACHOMETER*†

By E. B. BROWN, D.Sc.‡

(*Paper first received* 30*th March*, 1938, *and in revised form* 13*th September*, 1938.)

SUMMARY

The theory of a new principle for an electrical tachometer is worked out, and experiments made in verification of this theory are described.

The conclusion is reached that, working on this new principle, an electrical tachometer can be constructed which has several advantages over existing types.

INTRODUCTION

The quadrature tachometer was invented by the author in December, 1937. It has not been described previously, and works on a principle believed to be new.

A two-phase magneto generator with a stationary armature and revolving magnet or inductor is connected by wires to an indicating instrument resembling an ordinary dynamometer wattmeter.

The indicating instrument has a movable coil in series with a high resistance which is fed from one phase of the small generator. This movable coil carries a small current proportional to the speed and nearly in phase with the e.m.f. which produces it.

The indicating instrument has also a fixed coil which is fed from a quadrature phase of the generator. This coil and the quadrature phase of the generator are both wound so that this circuit has a reactance large compared with its resistance. The fixed coil thus carries a current which is nearly in quadrature with the e.m.f. producing it and which remains nearly constant at working speeds.

The currents in both fixed and movable coils thus remain practically in phase at all speeds, and a nearly proportional scale results.

The quadrature tachometer has a considerable advantage over the other forms of instrument utilizing alternating current, in that it gives a practically linear scale without the necessity of a rectifier.

Theory and experiment show that the readings of the instrument are practically unaffected by changes in resistance of the circuits such as might be caused by temperature change.

The theory is developed for sine-wave e.m.f.'s and currents, but experiment shows that even a very bad wave-form in the induced e.m.f. has no serious effect on the operation of the instrument.

THEORY

Referring to the above description and to Fig. 1, the connection diagram of the generator and indicating instrument:—

Circuit 1 consists of phase I of the generator, the connecting leads, and the potential circuit of the indicator.

Circuit 2 consists of phase II of the generator, the connecting leads, and the current coil of the indicator.

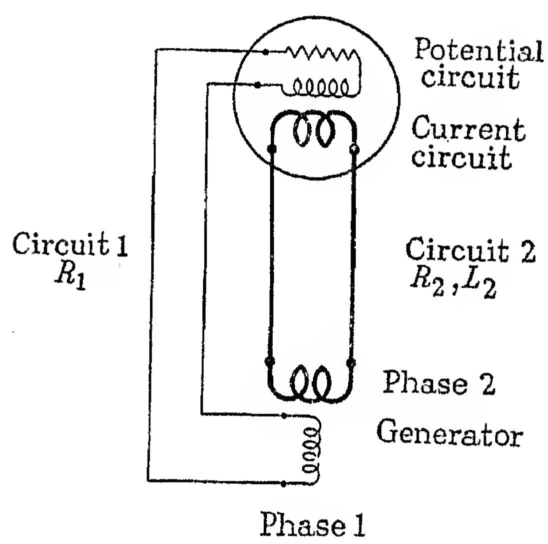

Fig. 1

Circuit 1. Total resistance R_1 ohms, assumed non-inductive on account of its high series resistance.

Circuit 2. Total resistance R_2 ohms and inductance L_2 henrys, impedance $Z_2 = \sqrt{[R_2^2 + \omega^2 L_2^2]}$, where sine-wave currents and voltages are assumed, and $\omega = 2\pi \times$ frequency of alternation, which is proportional to the speed, and ϕ_2 is the angle of lag, where $\tan \phi_2 = \omega L_2/R_2$.

Referring to the vector diagram, Fig. 2, using r.m.s. values, E_1 and E_2 represent the e.m.f.'s generated in phases I and II at a frequency $\omega/(2\pi)$, and I_1 and I_2 are the corresponding currents.

Then $$I_1 = \frac{E_1}{R_1} \quad \text{and} \quad I_2 = \frac{E_2}{Z_2}$$

The mean torque on the moving coil of the indicator is proportional to $I_1 I_2 \cos\left(\frac{\pi}{2} - \phi_2\right)$, i.e. to $I_1 I_2 \sin \phi_2$

* After the first draft of the paper had been prepared a small revolving-field 2-phase magneto was constructed which gives very satisfactory results in conjunction with the reflecting indicator. The construction of an iron-cored pointer-type indicator with pivoted coil was later completed, the instrument being used in conjunction with the 2-phase revolving-magnet generator. The scale of the indicator is graduated from 100 to 2 200 r.p.m. and is nearly uniform from 200 r.p.m. upwards. The apparatus is giving very accurate results. The total weight of the generator is 3 lb. 10 oz. and that of the indicator 3 lb. 6 oz. It is of interest to note that since the indicator is fitted with air damping, it contains no permanent magnetism and, therefore, will not affect a compass, an advantage for use in aircraft, as is also the small temperature coefficient.

† The Papers Committee invite written communications, for consideration with a view to publication, on papers published in the *Journal* without being read at a meeting. Communications (except those from abroad) should reach the Secretary of The Institution not later than one month after publication of the paper to which they relate.

‡ University of Melbourne.

or $$\frac{E_1E_2}{R_1Z_2}\sin\phi_2$$

Now, E_1 and E_2 are each proportional to ω

$$\begin{aligned}\text{whence the torque} &= \mu\frac{\omega^2}{R_1Z_2}\sin\phi_2\\ &= \mu\frac{\omega^3L_2}{R_1(R_2^2+\omega^2L_2^2)}\\ &= \frac{\mu\omega}{R_1L_2\left(1+\dfrac{R_2^2}{\omega^2L_2^2}\right)}\end{aligned}$$

Now, except for very small values of ω, R_2 is small compared with ωL_2, so approximately

$$\text{Torque} = \frac{\mu\omega}{R_1L_2}\left(1-\frac{R_2^2}{\omega^2L_2^2}\right)$$

and except at very low speeds the term $R_2^2/(\omega^2L_2^2)$ will be negligible compared with unity. Therefore the

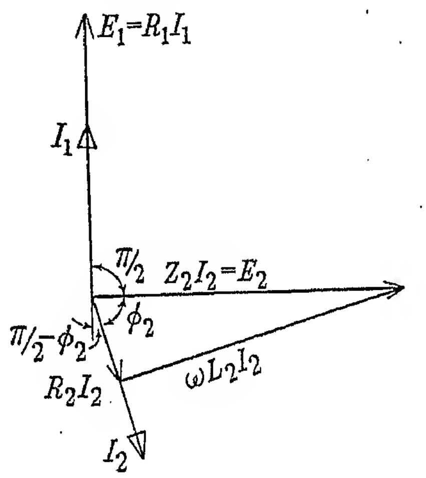

Fig. 2

torque $= \mu\omega/(R_1L_2)$, which is proportional to the speed and inversely proportional to the product of the resistance of circuit 1 and the inductance of circuit 2.

Thus, in the ideal case assumed, the instrument must give a linear scale over a considerable range. The readings will be practically independent of changes in resistance in the circuits such as may be caused by temperature change, for the following reasons:—

Variations in the resistance of the coils in circuit 1 produce no effect because of the high constant series resistance in this circuit.

Variation of the resistance of circuit 2 also causes no error, since if this resistance is small the torque is not dependent on its value.

It will now be shown that the slight residual temperature coefficient can be practically neutralized, if necessary, by giving a lead to the potential phase of the generator in excess of the quadrature position.

Using the notation previously given, let the machine or phase I have a total phase lead of $\frac{1}{2}\pi + \theta$ radians on the machine or phase II.

The modified expression for the torque T becomes

$$T = \frac{\mu\omega^2\sin(\theta+\phi_2)}{\sqrt{[R_2^2+\omega^2L_2^2]}} = \frac{\mu\omega^2}{R_2^2+\omega^2L_2^2}(R_2\sin\theta+\omega L_2\cos\theta)$$

By differentiating and simplifying we obtain

$$\frac{dT}{dR_2} = -\mu\omega^2\sin(2\phi_2+\theta)$$

If θ be such that at a particular speed, giving a phase lag ϕ_2, we have $\theta + 2\phi_2 = \pi$, then for a small increase in R_2 there will be no change in the indication of the instrument. At lower speeds $\theta + 2\phi_2 < \pi$, and at higher speeds $\theta + 2\phi_2 > \pi$, so that at lower speeds a small increase in R_2 causes a drop in the indication, and at higher speeds an increase in the indication. Thus

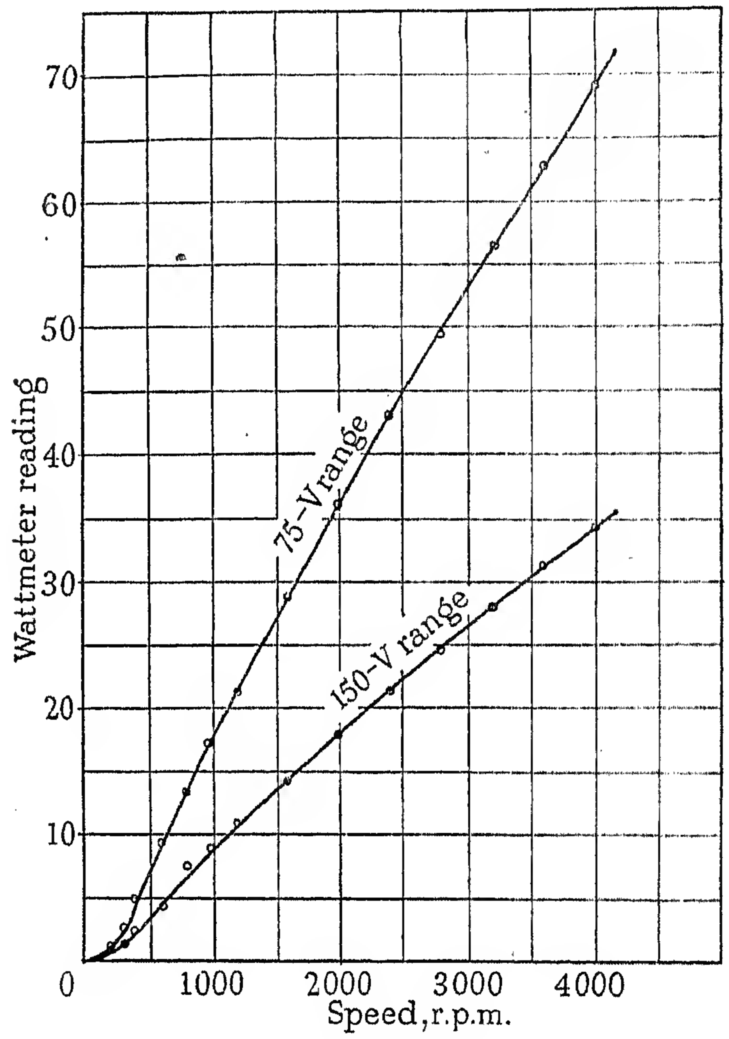

Fig. 3

it is possible, by adjusting θ, to obtain a zero temperature coefficient at a particular speed, a small negative coefficient at lower speeds, and a small positive coefficient at higher speeds.

In order to test the theory, since a 2-phase magneto was not available two Splitdorf inductor magnetos were rewound, No. 1 with 21 S.W.G. enamelled copper wire and No. 2 with 18 S.W.G.

The shafts were coupled together with the armatures initially approximately in quadrature.

For the indicating instrument a Weston 1-amp. wattmeter was used with 75-volt and 150-volt ranges, the potential circuit being connected to No. 1 and the current coil to No. 2.

By rocking the armatures the relative phase of the e.m.f.'s in No. 1 and No. 2 could be altered. It was found that increasing the lead of No. 1 generator feeding the potential phase produced an increase in the readings, indicating that the lag in circuit No. 2 was less than 90°.

The armatures were finally adjusted to give a maximum wattmeter reading, using the 75-volt range at 1 200 r.p.m., No. 1 then having a total lead on No. 2 of 126°, the armatures were locked in this position and calibration

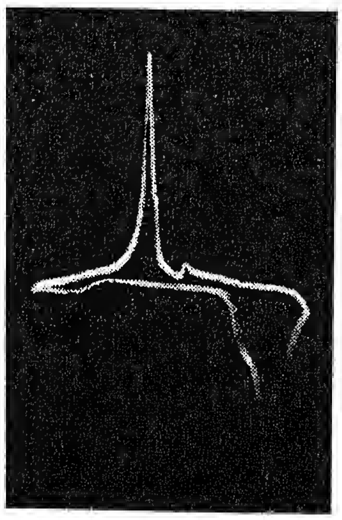

Fig. 4

curves (Fig. 3) were obtained. Indicated watts are plotted against speed.

FORM OF GRAPHS

It will be noted that the graph between indicated watts and speed is not quite linear but is quite satisfactory for a tachometer.

The ordinates of the graph using the 75-volt range are approximately double those for the same speeds using the 150-volt range, indicating that a satisfactory adjustment of the instrument is possible by varying the series resistance in the potential circuit. Graphs obtained on successive days appear to be consistent with each other.

A test was made to determine the effect on indications of variation of resistance of circuit 2. The total resistance of circuit 2 was 0·524 ohm, composed of the coils

Fig. 5

of phase II, the leads, and the instrument current coil. With the armature running at various constant speeds the resistance of this circuit was increased 10 % for each speed, which would correspond roughly to 25 deg. C. rise in temperature.

No appreciable alteration was made in the readings by this change of resistance, except for the very low values of speed.

WAVE SHAPES OF POTENTIAL AND CURRENT OF THE MAGNETOS

These were obtained by cathode-ray oscillograph. Fig. 4 shows the terminal potential of generator No. 1 running at 1 337 r.p.m.; it has a very sharp peak.

Fig. 5 shows the current wave in circuit 2, which also exhibits considerable departure from a sine wave.

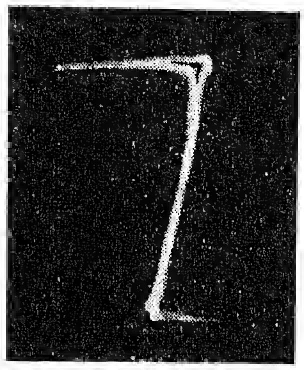

Fig. 6

In Figs. 6 and 7 the horizontal vibration is given by the terminal potential of generator No. 1, and the vertical vibration is given by the current in circuit 2. Fig. 6, taken at 1 356 r.p.m., shows that the current and voltage are closely in phase. Fig. 7 at 3 000 r.p.m. shows a slight phase displacement.

A test was made with the object of verifying the prediction from theory that the temperature coefficient of the instrument could be modified by varying the lead of the potential phase.

In order better to observe small changes in readings, a more sensitive indicator than the Weston wattmeter was used. This was an iron-cored moving-coil reflecting dynamometer instrument of which the winding of the laminated magnet formed part of the inductive circuit of generator No. 2. This circuit also contained an additional iron-cored reactance. Generator No. 1 and the moving coil with 20 000 ohms non-inductive resistance in series completed the potential circuit.

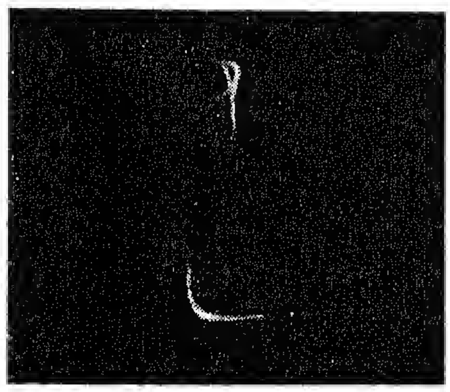

Fig. 7

A practically linear scale was obtained from 200 r.p.m. to 2 000 r.p.m.

The relative angular coupling of the two machines was altered until the indication at 1 200 r.p.m. was practically unchanged by an increase in resistance of the inductive circuit No. 2 by 12 % of its value.

A complete series of readings was taken both without and with this added resistance. The results are exhibited

in Fig. 8, which shows the change of indication in r.p.m. produced at different speeds by increasing the resistance of the inductive circuit by 12 %.

The form of curve obtained verifies the prediction that at one particular speed a zero temperature coefficient

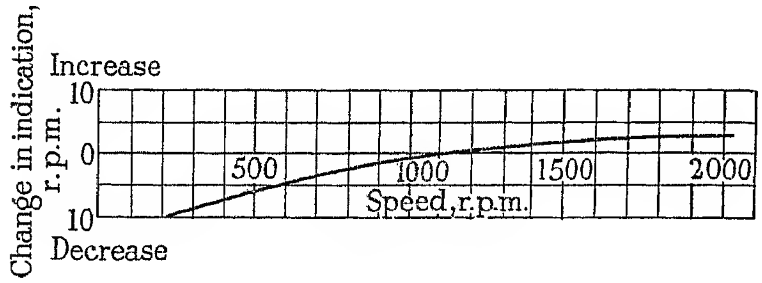

Fig. 8

can be obtained, for higher speeds a small positive coefficient, and for lower speeds a small negative coefficient, increasing as the speed becomes smaller.

In practice this increase in temperature coefficient at low speeds need not be a serious matter, because the effect at low speeds may be diminished by working with a higher frequency in the armature, obtained either by using a multipolar rotating magnet or gearing up a 2-pole rotor to a higher speed.

Since 12 % increase of resistance corresponds roughly to 28 deg. C. rise in temperature of winding, it is evident that even at low readings the temperature coefficient is very small.

Measurement of the total mechanical lead of generator No. 1 on No. 2 showed it to be 124°, that is 34° beyond the quadrature position, so that at 1 200 r.p.m. the lag ϕ_2 may be calculated as follows:—

$$2\phi_2 = 180° - 34° = 146°$$

Thus ϕ_2 was 73° at 1 200 r.p.m.

CONCLUSIONS FROM EXPERIMENTAL RESULTS

(1) The theory put forward is substantially correct, any slight discrepancies being easily accounted for by the abnormal wave-shapes.

(2) The very satisfactory results obtained from the crude apparatus employed encourage the belief that a very good tachometer may be constructed on this principle.

DISCUSSION ON "LINE PROTECTION BY PETERSEN COILS, WITH SPECIAL REFERENCE TO CONDITIONS PREVAILING IN GREAT BRITAIN"*

SCOTTISH CENTRE, AT EDINBURGH, 25TH OCTOBER, 1938

Mr. C. H. A. Collyns: The concern with which I am associated is one of the few in Scotland that has a Petersen coil in operation, and it has proved entirely successful. Of course, we had a few minor difficulties with it at first, but we quickly discovered the weak points on our system. When we installed the coil we turned out at midnight, wondering what was going to happen, and put a solid earth on one phase of the 11-kV system. Nothing happened for a short time, and then our instruments indicated that a switch had tripped elsewhere on the ring main. Inspection quickly discovered a faulty insulator. Then the lightning arresters started to break down, and had to be disconnected. We discovered and cleared off all our weak links within a period of 3 days, simply by creating artificial earth faults and bringing the coil into operation. One interesting fault was caused by a broken tail between the overhead line and cable box flapping clear and to earth alternately. The coil held that fault for about an hour before another weak link broke down, namely the end winding of a small transformer. This is the only occasion during the last 10 months on which we have had a transformer damaged by the effects produced by the coil.

The number of outages on the system this year has been reduced by some 60 per cent compared with the corresponding period a year ago. These outages are of course due partly to earth faults and partly to phase faults, but even the Petersen coil will not afford protection against, for instance, a tree branch blown over the 3 phase-wires. Within the last 10 months the coil has operated on 36 occasions.

We had rather an interesting experience a few weeks ago when a farmer drove a rick-lifter into the bottom phase wire. He saw a little spark, and wondered whether it was safe to touch the rick lifter. Finally he unyoked his horse and led it away. Our engineers got hold of the rick lifter and pulled it clear without experiencing any discomfort. There could have been but little potential there otherwise the horse would not have survived. A horse had been killed in similar circumstances prior to the installation of the coil, and on a different part of the system.

We are assured in the paper that the Petersen coil will prevent any trouble due to salt storms, and it would be interesting to have some further information on this point. It seems to me that when a salt storm is blowing across a 3-phase line and the salt is deposited on the insulators of all phases, if one phase breaks down to earth and the other phases are raised to a voltage equivalent to line voltage throughout, then they must immediately break down as well and cause a phase-to-phase fault.

All our early cables were intended for use with a solidly

* Paper by Mr H. Willott Taylor and Dr. P. F. Stritzl (see vol. 82, p. 387).

earthed neutral, and we rather wondered what would be the effect of the Petersen coil on these cables. We have had no trouble so far with cable breaking down, even in cases where the fault has held for a number of hours.

When we installed the coil we had some tests made to determine the effect of an out-of-balance due to a section of the overhead line being disconnected. We found that with the longest spur line that we could run with one fuse withdrawn we could get no reliable indication on the coil. Only with long lengths of cable does the switching-out of a section have any effect.

I should like to have information with regard to directional relays for indicating the section on which a fault occurs. The cost of these relays plus the special voltage transformer required to operate them on any large system is greater than the cost of the coil itself, and therefore it is doubtful whether it is necessary to put them in each section. We have had our coil working for 10 months, and have not yet found these relays to be necessary. We have one indicator on each outgoing feeder at the supply point only, but the difficulty is that these do not have time to operate on transient faults, and we either have to check over the system to find out the potential fault or wait until it recurs. Is it possible to make these relays function more nearly instantaneously?

I should like to refer to a rather interesting fault which occurred on our system—a pick hole in one of the l.t. cables. The cable in question is fed through a star/star transformer and a delta/star transformer on to the 11 000-volt system. The fault operated the Petersen coil. The e.h.t. system remained normal, but the l.t. fault indicated on the coil. I cannot understand the reason for this.

Finally, I should like to know why the Petersen coil is sometimes mounted on a concrete slab.

Mr. J. S. Pickles: In my supply area the high-voltage distribution network comprises about 600 miles of 11 000-volt overhead lines, and, although very severe weather is experienced, the lines are remarkably reliable except during lightning storms. These storms became a very serious factor and were the cause of numerous interruptions to supply. Usually there was no permanent damage to lines and equipment, but it became essential to do something to minimize the number of interruptions consequent on the tripping of switches or the blowing of high-voltage fuses on lightning spillovers. For the protection of 600 miles of lines the adopted scheme comprises four Petersen coils, two of 5 amperes, one of 9 amperes, and one of 23 amperes capacity. The total cost for the coils will be roughly £700, for recording equipments £200, and for installation £100. This makes a total cost of £1 000, or less than £2 per mile, at which rate it may be considered a very economical investment for the protection of such a large system.

One coil protecting 125 miles of 11-kV lines has been in operation for 6 months, and the recording chart shows that, in that period, the coil has cleared a considerable number of disturbances which would otherwise have resulted in interruptions to supply. On one occasion during a severe lightning storm the coil cleared 14 disturbances within an hour. In the other part of the area, not protected by a coil but subjected to the same lightning storm, there was a succession of interruptions. Owing to the fact that, on the occurrence of an earth fault, two phases are raised to full line voltage above earth, the Petersen coil soon finds any weak spots in the insulation of the system and this aspect has to be borne carefully in mind in considering whether the coil is practicable in any particular case.

One matter not mentioned in the paper is that it is necessary to get the consent of the Electricity Commissioners to depart from their existing regulation which requires the neutral point to be solidly earthed. The Commissioners' consent is readily forthcoming.

These coils are designed to protect a given mileage of lines and are commonly supplied with tappings to provide for a variation of about 20 per cent from the designed conditions; but in a large network there are frequent occasions when it is necessary temporarily to transfer mains from one point to another, the variation from the designed conditions being anything up to 100 per cent under these circumstances. During such periods the protection afforded by the coil is largely vitiated, and I should be glad to know whether any damage may then occur to the system or to the coil. I suppose that, under such conditions, the coil ought to be short-circuited, but it is not desirable that the operating staff should have too many operations to carry out in an emergency.

Mr. P. Butler: It is difficult to understand why the Petersen coil should be so wrapped up in mystery. It is simply an ordinary choke coil, and its design presents no difficulty provided that the proper precautions are taken. Under certain conditions difficulties may be experienced with Petersen coils owing to the existence of harmonics in the supply, and special precautions will undoubtedly have to be taken to compensate for these. It would be interesting to learn the authors' experience in this connection.

It is stated in the paper that most of the transformers which have been installed in this country will be capable of withstanding the excess voltage which is imposed on them when the Petersen coil operates. It is, however, advisable in all cases, where consumers are proposing to install Petersen coils, for them to write to the transformer manufacturers in order to obtain their views on this point. This is particularly important where Petersen coils are installed for continuous operation, since dielectric stresses set up due to the increased voltage may eventually cause breakdown.

With regard to Mr. Collyns's remark that when a Petersen coil was installed on his system the lightning arresters failed, it should be borne in mind that when the weak points have been found out and the strength of the system increased, the weakest link may then be a more vital piece of apparatus. It is quite possible, for instance, that if the insulation strength is increased throughout, the weakest link may turn out to be the transformers, and in the course of time transformer failures may develop as a result of installing the Petersen coils.

It would be interesting to learn what is the experience of the authors with regard to installing Petersen coils on a system equipped with star/star-connected transformers, assuming that the coil is connected in the neutral of the secondary side and that the neutral point on the primary is not connected to earth. Under these circumstances

there will be a very high flux value in the tank of the transformer, which will eventually cause breakdown. A number of star/star-connected transformers are still in operation, and in certain instances Petersen coils have been installed in conjunction with them. If the neutral on the primary side is connected through to the generator, or if there is a path round which the current can circulate, the conditions will not be so severe.

Another point which occurs to me is concerned with the installation of Petersen coils on star/star-connected transformers fitted with tertiary windings. It should be borne in mind that the capacity of the Petersen coil must be designed to match the capacity of the tertiary winding, since the fault current will flow round the delta. In two instances which have been brought to our notice this point was overlooked, with the consequence that the tertiary windings were completely burned out.

In one of the slides shown by the author the operation of the Petersen coil was indicated on the chart of a recording ammeter, the needle of which appeared to indicate that a constant current was flowing through the Petersen coil. It would be interesting to know why this current should flow, and its value.

We know that Petersen coils are not usually tuned to resonance, and in field tests which have been carried out over a range of positive tuning of 23 per cent and of negative tuning of 20 per cent no difference in extinguishing effect could be seen. In certain instances where the disposition of the conductors with regard to the surface of the earth is not entirely symmetrical, resonance tuning may actually be unsuitable, for the asymmetrical voltage has its maximum value when the network is protected by a Petersen coil tuned to resonance.

Mr. W. J. Cooper: I should like to ask the authors what size of system, expressed approximately in miles of cable, is small enough to make the use of a Petersen coil unnecessary.

I should like to mention that in the case of some of the small undertakings which are taking their bulk supply with the neutral earthed, a Petersen coil cannot be introduced unless a 1 : 1 transformer is installed or an arrangement is made with the bulk-supply authority to permit operation with insulated neutral.

It is rather surprising to learn that the Petersen coil has been so largely adopted on the Continent in comparison with this country. Perhaps the authors will tell us the reason for this.

[The authors' reply to this discussion will be found on page 506.]

FURTHER CONTRIBUTION TO THE DISCUSSION

Mr. V. Pickles (Transvaal) (*communicated*): In a previous contribution to the discussion (see vol. 82, p. 400) I mentioned that investigations were being made to ascertain the merits of Petersen coils on certain sections of the Victoria Falls and Transvaal Power Co.'s system. For the purpose of this test the 40-kV system was operated for alternate periods of 14 days with Petersen coils in service and with the neutral earthed. A very complete description and analysis of the data so far obtained have been embodied in a paper by Major E. F. Rendell entitled "Operating Experience with 80-kV and 40-kV Petersen Coils on the Victoria Falls and Transvaal Power Co.'s System" read before the South African Institute of Electrical Engineers and published in their *Journal* (vol. 29, page 138), but the following brief summary of the matter may be of some interest.

At the outset of the investigations it was considered that the best way of obtaining a true comparison of the two methods would be to operate the system for alternate periods of short duration, as this would tend to smooth out the effect of variations in climatic conditions and would also take care of changes introduced by extensions or modifications of the system. The test extended from 11th April, 1936, to 26th February, 1938, a period of 686 days, and during this time the line trips which occurred both during storms and in settled weather were carefully recorded. Table A sets out the number of disturbances that occurred in 343 days under each condition of working.

Assuming that the suppressions would have been trips with an earthed neutral, it will be seen that the number of disturbances including suppressions and trips under each condition of working were:—

With Petersen coils 271
With neutral earthed.. 303

Whilst these two figures are not exactly similar they indicate that, generally speaking, the climatic conditions obtaining during both methods of working were approximately the same.

The point is frequently made that Petersen coil operation tends to increase the number of multi-phase faults, but the results given in Table A do not support this view. Assuming the undetermined faults to be multi-phase faults for the purposes of this comparison, then under earthed-neutral conditions the percentage of multi-phase to total faults in stormy weather is 75 %. In Petersen coil operation the equivalent figure is 62 %. For fine-weather conditions the contrast is even stronger, the corresponding figures being 63 % and 37·5 %. For our

Table A

	With Petersen coils		With the neutral earthed	
	Storms	Fine weather	Storms	Fine weather
Suppressions ..	47	50	—	—
Line trips	128	46	216	87
Equivalent line trips	175	96	216	87
Nature of fault:				
Single-phase ..	66*	60†	53	32
Multi-phase ..	76	8	111	15
Undetermined ..	53	28	52	40

* Includes the 47 suppressions. † Includes the 50 suppressions.

present purpose therefore the number of disturbances that occurred with the neutral earthed may be disregarded and attention confined to the analysis of the observations recorded when the system was operated with Petersen coils.

The system comprises 415 miles of 40-kV overhead lines, of which 88 miles are constructed with pin insulators, 258 miles have disc insulators, and the remaining 69 miles are a combination of both types. There are 261 miles of line arranged as ring or interconnecting feeders, and 154 miles are non-ringed supply lines.

In order to give a clearer picture of the results, the figures of the first two columns of Table A have been sub-divided, and in Table B the number of trips affecting ringed and non-ringed lines are shown separately.

Examination of the figures gives the impression that the Petersen coils have been completely ineffective on non-ringed lines. Such an impression would not be correct, however, for the following reason. The non-ringed lines are fitted with a very sensitive earth-leakage protection, while on the ringed line a form of differential pilot-wire protection is employed. It is known that the earth-leakage protection invariably operates on a transient earth fault before the Petersen coil has suppressed that fault. This does not occur on ringed lines, consequently it has been assumed that single-phase faults on non-ringed lines have always led to a trip, although undoubtedly such faults would usually have been suppressed if a less-sensitive form of protection had been employed.

Table B

Comparisons of Fault Trips of Ring and Non-ringed Lines on Petersen Coil Operation

	Ringed lines		Non-ringed lines	
	Storms	Fine weather	Storms	Fine weather
Correct suppressions	47	50	—	—
Line trips	47	22	81	24
Phases affected:				
Single-phase ..	2	5	17	5
Multi-phase ..	36	5	40	3
Undetermined ..	9	12	24	16

It must be appreciated that when applying Petersen coil protection to an already established and extensive system, it is not practicable to modify or replace immediately existing types of protection unsuitable for Petersen coil installations, and it is unfortunate that this feature excludes the non-ringed lines from the analysis, making it necessary to confine the discussions to the results obtained on the ringed lines only.

The 7 single-phase faults shown in the first two columns of Table B were of a sustained nature, and since the Petersen coils should suppress all single-phase faults other than permanent ones, the 9 undetermined faults are classed as multi-phase. Thus it may be said that during lightning storms on 261 miles of ringed lines there were 47 correct suppressions, 45 multi-phase faults, and 2 sustained faults, while during fine weather there were 50 correct suppressions, 17 multi-phase faults, and 5 sustained faults. The general conclusion of this investigation is that on a system such as the 40-kV system operated by the Victoria Falls and Transvaal Power Co., the use of Petersen coils will eliminate approximately 50 % of switch trips during lightning storms, and about 70 % during settled weather.

Another part of the system equipped with Petersen coils is a section of the 80-kV main transmission system. This section comprises one single non-interconnected line 127 miles long and four interconnected transmission lines, each averaging about 40 miles in length, and the operating records, covering the period 16th June, 1937, to 26th February, 1938, are presented in Table C.

Table C

Performance of 80-kV Petersen Coils for 225 days

	Lightning storms	Fine weather
Suppressions	27	46
Line trips	9	1
Total fault occurrences ..	36	47
Correct suppressions, % ..	75	98

Phases Affected on Line Trips

	Lightning storms	Fine weather
Single-phase	2	–
2 phases	6	–
3 phases	1	–
Undetermined	–	1
Proportions of multi-phase and undetermined to total, % ..	19	2

Of the 47 fine-weather occurrences all, except one, were of a single-phase nature and due to birds. The reduction in line trips is 75 % during lightning storms and 98 % during settled weather.

As will be seen, the use of Petersen coils is more effective on the higher voltage section of the system, and this is to be expected by reason of the higher insulation level and thereby a reduced tendency to multi-phase faulting.

The outstanding feature of the analysis of the 40-kV system records is the large proportion of multi-phase faults which occur during lightning storms. This is very largely due to the higher footing resistances of the towers resulting in the higher pole voltage whenever a tower or guard wire is struck, which causes more than one phase to flash over.

In conclusion it may be said that on a system such as that under discussion, where arrangements exist for duplicate supplies to the great majority of consumers and where the overhead lines are constructed so as to be practically immune from permanent damage, combined with auto-reclosing equipment at both ends, the trip of any one line is not of great importance in so far as system reliability of supply is concerned. For this reason it must not be assumed that the diminution in the number of line trips resulting from the use of the Petersen coil is a measure of the increase in the system reliability.

The figures given represent an attempt to show the advantages of Petersen coils, and, generally speaking, while, as already stated, the reduction in the number of trips is not a measure of increase in system reliability, this apparatus may be regarded as one worth investigating when the object is to improve the performance generally.

The method of tuning coils to line-length conditions adopted on the Victoria Falls Power Co.'s system is by off-load tap-changing, the Petersen coil being short-circuited during the process. As this interval is so short, it is felt that there is little to be gained by resorting to the principle of on-load tap-changing.

THE AUTHORS' REPLY

Mr. H. Willott Taylor and **Dr. P. F. Stritzl** (*in reply*): As in previous discussions, the comments can be divided into two distinct categories, i.e. those dealing with actual experience of Petersen coils in operation, and others dealing with points of a more theoretical nature.

We record with satisfaction that Mr. Collyns reported operating experience substantially confirming our own. He quotes interesting figures which show the reduction in supply interruptions on his 11-kV network. In addition, he has provided several interesting examples of fault conditions to be met with in practice on an overhead network. We agree that commissioning tests will speedily disclose any latent weak points in a system, but we wish to comment on the breakdown of surge arresters which he has experienced when an artificial earth fault was applied. As far as we are aware, the surge arresters referred to were, in fact, arc-gaps without current-limiting resistances, a device hardly deserving the name given to it by Mr. Collyns. Like other obsolete types of surge arresters, these gaps are a source of trouble rather than a safeguard, and the sooner they are removed the better. Modern-type surge arresters, such as have been made by the world's leading electrical manufacturers for the past 8 years, cannot conceivably cause any trouble of this nature; indeed, they are looked upon as a necessary supplement to the Petersen coil, inasmuch as they fulfil a function not covered by the coil, i.e. the protection of apparatus against initial breakdown of insulation due to surge voltage. In other words, the coil does not come into operation until a fault has occurred, when it has the effect of limiting the amount of damage sustained by the apparatus. The function of the arrester is merely to prevent this initial breakdown occurring on substation apparatus. Of course it is necessary for arresters used on an insulated-neutral system to have a higher voltage-setting to prevent their operation when the voltage of the healthy phases rises to the line voltage.

With regard to the question of salt contamination, the time-lag of insulation breakdown on an insulator carrying a salt deposit appears to be ample to enable the coil to clear a transient fault (which type of fault has been shown to form the major portion of all faults encountered on an overhead line) in the normal manner, before another flashover occurs to cause an interphase fault. Even in the case of sustained faults, experience shows that a properly insulated line is practically immune from double earth faults.

We are interested to have further confirmation of our contention that cable designed for earthed-neutral working is entirely suitable for operation in a coil-protected system.

The statement as to the cost of equipping a system with directional earth leakage relays to assist in the location of faults requires some qualification. It does not seem quite fair to us to compare the cost of these indicators with that of the coil itself. The number of relays considered necessary in any particular network depends entirely on local conditions, on the operating routine, and on the nature of the load, and it frequently happens that a small coil is used, together with a great number of indicators, whereas a large coil may be worked with few relays or none. It is usually very easy to find a mean between technical ideals and practical economy. On the average the cost of relays and auxiliary equipment will be found to form only a small percentage of the cost of the coil itself. Even with these accessories included, the fact remains that, in terms of protection offered, the coil compares most favourably on the score of cost, with any other protective device. Mr. J. S. Pickles's opening comment is an illustration of this point.

The operating time of indicating relays is easily made so short as to make them indicate transient faults. Since, however, a fault on a coil-protected system only becomes of any importance when it takes on a permanent nature, the majority of users prefer the relays not to indicate transient flashovers. To achieve this aim, the operation is deliberately retarded. This practice prevents them from responding to conditions which similate the characteristics of an earth fault, such as, for instance, the non-simultaneous closing of the contacts on a triple-pole switch.

We suggest, in the absence of more detailed information, that the case mentioned by Mr. Collyns, of l.t. faults causing a Petersen coil connected to the complementary h.t. system to operate, has its explanation in the fact that the high unbalanced fault-loading on the l.t. side may so distort the h.t. voltage relationships as to cause a current to flow in the coil. This would presuppose a coil tuned very near to resonance, which case would rarely arise in practice. We presume, further, that this type of fault was only recorded by the general alarm device of the coil, but not by any of the directional indicators; thus, the l.t. fault was probably distinguished from an h.t. earth fault, as in the case of the former there is no zero phase-sequence component in the current to operate the relays.

A Petersen coil is usually a fairly small piece of

apparatus and may be mounted upon a concrete plinth in order to render inaccessible the neutral connection, which may be at a high potential while an earth fault is being dealt with.

Mr. J. S. Pickles confirms that any existing insulation weakness is soon revealed when commissioning a coil, but it should be borne in mind that trouble would eventually arise at such points, even when running with earthed neutral and then with unpleasant consequences, so that suspicion of their existence should not be allowed to influence a decision to install coils in any particular instance. As Mr. J. S. Pickles remarks, no opposition is raised by the Electricity Commissioners to the installation of Petersen coils.

It should be pointed out that although the efficacy of a coil is reduced should it, owing to operating necessity, be very much out of tune, nevertheless, a measure of protection is always preferable to none.

We consider it, from our own experience, highly improbable that the variation from designed conditions can, at any time, approach such a high figure as 100 %; if it does, the coil would, in all probability, be transferred together with the lines it is to protect. In other words, we do not think the question in the form in which Mr. Pickles presented it is likely to arise. We agree with him that the operating staff should not have too many operations to carry out in an emergency, and we consider it unnecessary to provide a short-circuiting switch for use in such an emergency.

We must admit being somewhat at a loss in face of Mr. Butler's remark as to the mystery surrounding the Petersen coil, since a study of the works listed in the bibliography attached to the paper should alone suffice to dispel any uncertainties associated with the subject.

Harmonics may be found in a system and cannot, of course, be eliminated from the residual fault current by a coil tuned to fundamental frequency, but they are hardly ever present in a sufficient proportion to endanger the arc-suppressing properties of the coil. Methods of reducing their magnitude include the installation of a circuit tuned to the particular frequency to be removed, or, alternatively a method of voltage injection into the faulty conductor. Considerable success has also attended the scheme of applying a solid earth in a substation to the faulty phase, to which we have referred in the paper.

Mr. Butler's remarks confirm our own views expressed above regarding surge arresters, but we cannot see any connection between the installation of a Petersen coil and the damage to a transformer caused by a surge.

Mr. Butler then mentions the fact that an earth fault causes a high flux in the tank of a star/star-connected transformer. This is, of course, a well-known fact and limits the size of coils that may be connected to transformers in star/star connection to about 20 % of the transformer rating. In the event of three star/star-connected single-phase transformers a Petersen coil cannot be connected at all.

We should like to take this opportunity of clarifying our reply to Mr. Gray in the discussion at Chester (vol. 83, page 720). We there said that any transformer containing a delta winding is suitable for the connection of a coil, provided a certain rating is not exceeded. This is also valid for banks of single-phase units, provided there is a delta-connected primary or tertiary winding. Our subsequent statement, as to a bank of three single-phase transformers or a shell-type transformer being entirely unsuitable, referred, of course, to star/star connection without a tertiary winding.

The rating of the delta-connected tertiary winding with which the transformer may be fitted determines the permissible coil size. In the instances cited, this fact was probably overlooked and coils connected to transformers that were not suitable for the particular size of coil. It is necessary to investigate this side of the problem in every case before a coil is installed.

An asymmetric voltage may appear at the neutral point of a system under normal conditions as a result of capacitive unbalance in the phases, arising perhaps from an unequal length of single-phase spur lines being connected to the three phases, or from an inherent unbalance in particular transformer windings. With a coil tuned to the resonant point, this may drive a constant current through the coil and thus account for the phenomenon observed by Mr. Butler.

Replying to Mr. Cooper we would say that the factor which limits the size of network that can safely be operated with a fully insulated neutral below which the installation of a coil is unnecessary and therefore unjustifiable, is, of course, the value of the resultant earth capacitive current which would be just sufficient to enable an arc to restrike. Practice shows this value to be in the region of 3 amperes, which is provided by an average 6·6-kV 0·1-sq. in. belted cable system some 3 miles long, the figures for 11 kV and 33 kV being approximately 2·5 miles and 1·5 miles respectively.

The contrasting use made of Petersen coil earthing on the Continent and in this country has its origin mainly in the fact that in the former countries solid earthing of the neutral has never been practised as in Great Britain, and hence no rooted tradition stood in the way of the inductive neutral connection. On the other hand, the free neutral brought in its train the dreaded arcing-earth and stimulated efforts to remove this disadvantage without the necessity of interrupting the supply.

We are glad to note in the communicated remarks of Mr. V. Pickles that experience on the Victoria Falls and Transvaal systems has shown that the installation of Petersen coils has led to a diminution rather than an increase in the number of multi-phase faults as is sometimes mistakenly alleged.

We are also pleased to note that their experience confirms our view that there is no necessity for employing coils fitted with on-load tap changing.

With regard to the operation of existing earth-leakage trip coils on transient earth faults, Mr. V. Pickles does not explain the reason why they were not made rather less sensitive or removed altogether, as this would appear to have been the obvious course to follow.

We agree that on a system composed of ring mains the installation of a Petersen coil may not produce a very marked reduction of interruptions of supply, but it will certainly have the effect of saving the system from being subjected to unnecessary strain due to the single-phase short-circuits which can be expected to occur more or less frequently.

DISCUSSION ON

"HIGH-SPEED PROTECTION AS AN AID TO MAINTAINING ELECTRIC SERVICE FOLLOWING SYSTEM SHORT-CIRCUITS"*

NORTH-WESTERN CENTRE, AT MANCHESTER, 1ST NOVEMBER, 1938

Mr. H. G. Bell: When this paper was discussed in London, Mr. Marshall mentioned that on the grid as a whole about 86 % of all the faults could be cleared without serious disturbance to the system. This figure is obtained by comparing the number of system faults in which perfectly correct isolation by protective gear has been obtained, with the total number of system faults; so that the figure for correct relay operation as such would be higher.

It will be appreciated, for instance, that a single clearance of a fault on a lock-in circuit may involve the correct locking of perhaps three or four adjacent circuits, so that to obtain a figure of 86 % on the basis chosen represents a very high level of relay performance. This is only possible with soundly designed equipment which is very carefully maintained.

Reference is made by the authors to the effect of surges. On systems connected to the grid there are bound to be surges, due not only to grid faults but due to faults on systems of other undertakers connected to the grid. In the past these surges have in many cases been marked by loss of load, although there has been no interruption of the supply. This loss of load is generally due to the tripping of motors by instantaneous no-volt trips, or in some cases instantaneous overcurrent trips.

In many cases it has been possible to provide time-lags of up to 3 or 4 sec. on no-volt coils, and this has completely cured unnecessary tripping on voltage surges while still causing the disconnection of the motor in the event of a complete interruption in the supply. In some cases the fitting of time-lags on no-volt coils has necessitated revision of the overcurrent settings. Difficulties sometimes arise in applying time-lags to no-volt coils, owing to the fact that the same coil may be used for emergency trips, being operated by knock-off push buttons mounted on the driven machinery. Such a case may necessitate the provision of two separate coils if there is sufficient space in the starter.

In the application of no-volt coils with time-lags it is desirable to differentiate between "vital" motors and those of less importance. By "vital" motors I mean those driving machines involved in continuous manufacturing processes, where an interruption means damage to the material under treatment and possibly hours of cleaning-up before the process can be resumed. A time-lag on a no-volt coil of such a motor is essential, but where a stopped motor can easily be restarted, without serious effect on production, an ordinary no-volt coil may prove satisfactory.

Regarding pilot and carrier schemes, I am a great believer in schemes of the lock-in type, but means must be provided for a continuous or frequent test of the effectiveness of the locking channel. As it is not always practicable to have maintenance staff present simultaneously at the two ends of the protective circuit, it is generally necessary to provide means for testing the continuity of the lock-in circuit from some central point. In the case of the Central Electricity Board, this type of testing is carried out from the central control room in each area.

By considering the flag indication on modern relays a certain amount of information as regards the type of fault can be obtained. In the case of long overhead lines, however, it would frequently be an advantage to know also the location of the fault, and if flags were provided on the separate elements of the high-speed impedance relays described in the paper a rough idea of the position of the fault would be given. We have a number of beam-type impedance relays in service, but in the absence of flags there is no indication that these have ever operated.

Two general points in connection with protective relays may also be mentioned. One is the difficulty of obtaining reliable reports of flag indication. Anything that can be done in the design of relays to make flag indicators obvious and easily recognizable should be done. At present on two relays of different manufacture one may indicate by the appearance of a white disc whilst the other indicates by the disappearance of a white disc. The necessity for standardization is obvious.

Finally, there is the question of complexity. So many developments in protective gear consist of additions to already complicated equipment, each of which means another element to be maintained and another unit for which spares must be carried.

Mr. H. A. Lamb: When dealing with the subject of back-up of protection the authors recommend that overcurrent relays should have a considerable time-delay. Can they expand that statement a little and say what order of time they consider is safe in a general case? They indicate that the impedance type of protection for back-up purposes is to be preferred, and state that a setting for two feeder-lengths is desirable. Will they say on what grounds they recommend the two-feeder length setting? Is it merely to extend the operating zone of the back-up protection, or has that setting some special significance in relation to stability?

In regard to the high-speed protection shown in Fig. 10, will the authors indicate where the pilots are earthed, and can they say approximately what is the order of magnitude of the current in the pilot circuit for operation with normal full-load current on the primary circuit? How would they expect the system to perform under the con-

* Paper by Messrs. T. W. Ross and C. Ryder (see vol. 83, p. 228).

dition of a fault occurring on an adjacent feeder which might induce circulating currents in the pilot system of the high-speed protective gear? The beam relay would seem to be liable to false operation under such circumstances.

The last system of protection mentioned is the 3-step high-speed impedance system with locking on the instantaneous zone to give instantaneous tripping over 100 % of the first feeder-length. This system provides its own back-up protection, and would appear to be superior to the other unit systems in that respect. Do the authors concur, and do they claim that no additional back-up is required when this system is employed?

Mr. S. R. Mellonie: The paper indicates what degree of assistance is to be expected from high-speed relays and high-speed oil circuit-breakers to preserve the stability of the system when faults of major consequence occur. Will the authors please say whether any of the half-cycle relays which they mention have been in use for any length of time, and whether they have been proved to stand up to the abnormal phase relationship which must exist during the first few cycles following a major fault?

On page 231 the authors advocate, as the ideal protection for a motor, a device which is responsive to the negative phase-sequence currents only. Is such an elaborate system of protection justified in view of the very few cases of single-phasing which occur? I take it that this is the only advantage of negative phase-sequence protection over zero phase-sequence protection, which is so much simpler and cheaper.

With regard to Appendix 2 a word of warning is justifiable. The expressions for the synchronizing power of an interconnector are similar to those obtained by previous investigators,* but it must be emphasized that the maximum power which can be transmitted over a line cannot be ascertained from the line characteristics alone. It depends to a considerable extent upon the characteristics of the machines and the load. Furthermore, the discussion is limited to a simple impedance line, i.e. one that is electrically short.

The authors deduce the fact that a small impedance is necessary for a large synchronizing power. At first sight it would appear to be reasonable to decrease the impedance by adding parallel lines, but other factors exert a limiting influence in the case of long overhead lines. This is well illustrated by the curves published in a paper by Nickle and Lawton,† which show that with a 300 000-kVA station connected to three 220-kV 250-mile lines it is possible to transmit 200 000 kW, but with four lines only 190 000 kW; and if the station is connected to nine such lines it is not possible to transmit any power at all over those lines.

Before leaving the subject of high-speed protection I would point out that the modern cartridge fuse can interrupt faults even more quickly than these modern relays.

In the early days of the operation of the grid system considerable trouble was experienced due to very important motors being switched out because of the operation of no-volt coils. A trial period was instituted with some of the no-volt coils eliminated, and quite satisfactory operation was obtained.

* L. Romero and J. B. Palmer: *Journal I.E.E.*, 1922, vol. 60, p. 296.
† *Journal of the American I.E.E.*, 1926, vol. 45, p. 864.

Mr. M. Kaufmann: The authors make it clear that the real intention of high-speed protection is to exclude the danger of loss of continuity of supply as the result of instability following short-circuits. My impression was that this particular phenomenon is comparatively rare in Great Britain, but is prevalent to a much greater extent in America. Perhaps the authors can make it clear that high-speed protection has other advantages apart from the prevention of loss of continuity of supply. One is that the burning-through of conductors is eliminated by the much quicker clearance of faults; and the other, already emphasized by Mr. Bell, is that the risk of motors tripping off when the voltage falls, and of synchronous plant in general falling out of step, is also eliminated by high-speed fault clearance. Although in the past we have had clearances which were fast enough to prevent that, all of them involved pilot wires, which are not very practicable on long lines, for which this type of equipment (the high-speed "lock-in" system), and also high-speed impedance protection, are particularly suited.

I would point out that the application of time-lags to existing motors is not a very cheap matter, although it may sometimes be quite practicable. It generally involves adding some kind of dash-pot device to the no-volt coil itself, or else the addition of a shunt tripping device with a battery and a time-lag relay. All this is not cheap, and the result is that many motors will remain in existence with no-volt coils not fitted with time-lag devices, and high-speed fault clearance is going to enable people to keep those motors on their systems.

Perhaps the authors could give some details of the recent tests of a type of high-speed protection which embodies high-speed carrier equipment.

Although on the grid the lower-speed types of protection have managed to give as good a performance as 86 % correct operations, it is going to be very much harder to achieve the same high degree of correct operation with equipment of the kind described in the paper, in view of the fact that the complete operation, from the initiation of the tripping impulse to the actual opening of the circuit-breaker, is over in 8–10 cycles.

Mr. O. Howarth: I take it that the first three lines of the paper, about the interconnection of generating stations by overhead networks, really refer to the grid. It has been the experience of at least one undertaking in the Manchester district that grid faults are not of such serious consequence to the undertaking as might have been expected. This is due not so much to the high-speed relaying as to the fact that there are transformers between the generating stations and the grid, and so the reduction of voltage on the station busbars is nothing like so great as it sometimes is when faults occur on the undertaking's own system.

In the section dealing with "System Stability" (page 229) the authors mention that when a 3-phase short-circuit is applied across the terminals of a generator its output increases. I have checked this with a generator of 20 % reactance, and it seemed to me that the output would not increase above the full load if the generator terminals were short-circuited.

Dealing with the question of system stability, when a fault does come on it affects both stations, even if it is on the busbars of only one of them, and it tends to affect

a distant station more seriously than a station in close proximity; because whilst there is less actual current at a distant station there is usually much more power output, owing to the resistance drop in the cable between the station and the fault. I have heard station engineers complain about distant feeders being much more troublesome than adjacent feeders. This was due, of course, to the fact that when the short-circuit was close up to the station it did not impose a lot of additional load on it, partly because the existing load on the station was reduced on account of the reduction of voltage.

The curves in Figs. 2 and 3 seem rather incomplete, as no mention is made of the fault-loop resistance, although this is quite as important as the reactance.

Dealing with negative phase sequence protection of motors, would not this be likely to trip every time there was a phase short-circuit on the system? Do the authors advocate the use of some form of time-delay?

I should like to know whether the authors have had much experience of the system of transformer protection shown in Fig. 9. Why is the current transformer used in the secondary of the main current transformer in Fig. 10 to energize the restraint coil on the beam relay? What is the objection to carrying the secondary current right round that coil?

A curve of tripping values is given in Fig. 11 for the type of protection shown in Fig. 9. Can the authors say what percentage of full-load current the tripping current would be?

Mr. W. A. Crocker: Where the power line acts as a pilot by means of carrier current, what is the effect of a breakage in the power line on the protection? On pages 236 and 237 there is a statement to the effect that many installations of this type are now operating successfully in America. Does this mean that no such carrier system is operating successfully in England? About 10 years ago a paper* was read before The Institution on the subject, and I understand that lines with carrier-current protection have since been put into commission here.

A suggestion has been made that all no-volt releases on motor starters should be fitted with time-delay dashpots: I hope that such time-delays will not be obtained by the use of dashpots, which are, in my opinion, very unreliable.

Mr. C. F. Tyrrell (*communicated*): I should like to ask the authors to discuss the effect of the high-speed breaking they propose with regard to voltage-rise on the secondary side of transformers of systems on which the high-speed breaking occurs. The information in my possession seems to indicate that serious voltage-rises can and do occur.

Messrs. T. W. Ross and **C. Ryder** (*in reply*): We fully agree with Mr. Bell that to assess the efficiency of the protective gear on a network by comparing the number of correctly-switched faults with the total number of faults is hardly fair to the protective gear. It would be more reasonable to assess the efficiency by comparing the total number of relays carrying fault current which functioned correctly with those which functioned incorrectly. When one considers the enormous number of links in the protective chain on a large network it is surely a very creditable performance on the part of those designing, installing, and maintaining the gear, that it does so much to ensure continuity of supply following a short-circuit.

We are pleased to note that Mr. Bell supports the use of time delay on no-volt coils, but we feel that it is undesirable to use a no-volt coil for emergency trips. Such trips must be quick acting and would therefore defeat the object of the delayed action. Emergency tripping should be performed by mechanical means or by shunt trip-coils.

It is somewhat difficult to deal in a general sense with the subject of back-up protection, because there are so many valuable factors which must be considered before the most suitable arrangement can be arrived at. Time-delay overcurrent relays sometimes fail to give the desired protection owing to the decaying of the generator current when there is comparatively little reactance between the machine and the short-circuit. This might be overcome by reducing both the time and current settings, but such a procedure would make the relays more likely to operate on surging between generating stations. If overcurrent relays are under consideration a compromise will have to be made in adjusting both the time and current settings, but with distance-type back-up protection such a compromise is seldom, if ever, necessary.

In reply to Mr. Lamb, the operating zone of the distance relays is extended to cover a failure of the circuit-breaker controlling the next section and has no significance in relation to stability. We do not consider it necessary, and it is certainly not advisable, to earth the pilot wires used for protective gear, and in order to avoid this we introduce auxiliary current-transformers to insulate the pilot wires from the line current-transformers. The magnitude of the current in the pilot circuit is negligible during healthy conditions, and is of the order of 0·05 ampere for operation of the relays during fault conditions. Since the pilot wires are not earthed there is practically no possibility of an induced current flowing through the relay coils from an adjacent feeder.

We agree with Mr. Lamb that distance relaying with carrier or pilot locking usually includes a distance-type back-up element. This element is not essential for unit protection and could, of course, be added as a back-up feature to other locking schemes if so desired.

Mr. Mellonie asks whether any of the half-cycle relays have been in use for any length of time. The answer is that several equipments have been in use for several years on the 132-kV grid in this country and also on similar schemes abroad. The results in service are highly satisfactory, and the disturbances caused through short-circuits have been reduced to a very marked degree.

His remarks on the transmission of power are of interest but do not strictly come within the scope of the paper. The particular case quoted by Mr. Mellonie represents a condition which is somewhat abnormal, but we are grateful to him for bringing out the point. The explanation of this apparent anomaly is that the additional charging current due to the extra lines necessitates such a low generator field-current as to make the machine unstable.

Mr. Kaufmann makes the suggestion that additional advantages can be claimed for high-speed fault clearance in regard to the minimum of damage to conductors,

* *Journal I.E.E.*, 1930, vol. 68, p. 801.

the reduced risk of motors being tripped out, and synchronous plant falling out of step. The two latter have already been mentioned in the paper, and we agree that the former is a natural result of quicker fault clearance.

He asks for details of recent tests carried out on a high-speed carrier equipment. These tests were carried out some months ago, by and through the courtesy of the Central Electricity Board, on a 132-kV transmission line between Southport and Ribble. The protective gear comprised high-speed overcurrent and directional relays with a carrier communicating channel superimposed on the 132-kV line as described in the paper. Actual faults were switched on to the 132-kV system to determine the operation of the gear under:—

(*a*) Internal faults, both phase to earth and phase to phase, to check tripping.
(*b*) External faults of a similar nature to check stability.

The tests were the first full-scale demonstrations carried out on this class of gear and very successfully indicated the practicability of the scheme.

We are not in agreement with the suggestion that it will be harder to achieve the same high degree of correct operations with high-speed apparatus. Actually the relative discriminating tolerances with high-speed relays are greater than they are in schemes using slower operating relays.

Mr. Howarth refers to actual experience in the Manchester district indicating that primary faults in the grid do not cause serious disturbance. This is due to the "buffer" effect of transformers, although the disturbance depends to some extent upon the connected transformer and generating plant at the time of the faults, and it may be dangerous to draw general conclusions. The question of generator output when short-circuited depends upon the time interval being considered. Obviously the kW output under steady conditions cannot exceed the steam input. At the instant of the short-circuit, however, the machine flux is very much greater than it is under the steady state condition, and this is responsible for the increased output during the initial portion of the decrement curve. At the same time the change in phase angle results in loss of kinetic energy, which is also passed into the electrical circuit. For the purpose of discussing high-speed protective gear we are only concerned with the conditions immediately following the instant of short-circuit, and it is during this period that the increased output occurs. The point regarding a distant fault being more troublesome than a near one is not wholly dependent upon the distance. The amount of generating plant on either side of the fault and the nature of the synchronous tie that is left have a large bearing on the matter.

In computing the curves in Figs. 2 and 3, due allowance has been made for resistance. The reactance of the fault loop, however, is much greater than the resistance and is more widely used.

Negative phase-sequence relays for motor protection incorporate a time-delay feature, the characteristic generally being inverse. The latter, while preventing operation due to system faults, possesses the advantage of quick clearance in the event of a fault occurring in the protected motor.

The auxiliary transformer shown in Fig. 10 is for summating purposes when used on a 3-phase circuit. The tripping current for the scheme shown in Fig. 9 is 50 % of full load.

Mr. Crocker asks what is the effect of a breakage in the power line when it is used as a communicating channel by means of carrier currents. Under this condition the power circuit should be tripped, and the fact that the carrier channel is severed is unimportant, because it is not used for tripping purposes.

The carrier systems referred to by the authors are of the high-speed type and are therefore in a different category from the equipment mentioned as having been installed here.

In reply to Mr. Tyrrell we would point out that the opening time of 0·06 second of the high-speed breakers is not fast enough to cause excessive voltage-rise on transformers. Actually, with these circuit-breakers the current is interrupted nearer to the zero part of the cycle and therefore any voltage-rise would tend to be lower than with the ordinary type of circuit-breaker.

DISCUSSION ON

"THE CENTRALIZED CONTROL OF PUBLIC LIGHTING AND OFF-PEAK LOADS BY SUPERIMPOSED RIPPLES"*

SCOTTISH CENTRE, AT GLASGOW, 8TH NOVEMBER, 1938

Mr. H. C. Babb: In the Summary the author says that superimposed high-frequency signals may be transmitted on any distribution network; presumably he means combined high-voltage and low-voltage networks. To be useful, any form of centralized control must be really centralized, i.e. one must be able to apply it to e.h.t. networks, and the wave must be capable of filtering through various transformers to the low-voltage distribution in towns and villages.

A series emission ripple, as described on page 830, would appear to fulfil these desiderata. I do not think it is quite clear, however, why the growth of the system reduces the impedance. One would have thought that lengthening the feeders would have increased the impedance. If the feeders are in parallel, i.e. if they are connected at their extremities to form a ring main, the impedance will be reduced by the growth of the system, but the author states that each outgoing feeder must be separately energized, and that the feed for a ring main must be separately and simultaneously rippled. In considering rural districts and distribution, the systems will in the main be spur lines, and it seems to me that the system impedance will certainly tend to increase rather than decrease.

On page 831 (vol. 83) the author discusses the necessity of steady voltage. Has anything been considered on the lines of a Ferranti voltage regulator and astatic relay with a view to securing a uniform ripple?

Again, when it is desired to subdivide a large area into two or more districts, each controlled by a separate ripple master control, what will be the effect of a series of ripples for one district being superimposed on that of a neighbouring district? Will the resultant wave-form be inoperative on the receiving relays, because one can picture a composite wave-form which might result in, say, street lighting being switched on instead of air-raid sirens?

I am not clear as to what method would be employed to prevent an intermingling of ripples from two separate but interconnected districts.

I should be glad if the author would state whether the choke in the earth-return system connection affects any instruments such as earth recorders, or switchgear leakage apparatus, that may be at present in use; also, whether a Petersen coil connected to a combined network would affect the ripple system.

Mr. A. F. Stevenson: The author's curve (Fig. 5) showing the ripple superimposed on the ordinary frequency reminded one so much of the harmonic-ridden curves of the early grid days that one would have expected the relays to have been operated by other disturbances on the system.

The paper does not sufficiently stress the importance of ripple control of consumers' meters. This device has solved the problem of maximum demand at off-peak periods, a problem which has troubled us since the beginning of the century and is still the cause of much annoyance and injustice.

Mr. F. C. W. Clark: In connection with two-part-tariff meters I think that an admirable application could be made of ripple control for the operation of the fixed-charge-collector attachment. In the meters of this type at present in use a motor drives the fixed-charge attachment, and the gear ratio is of the order of $1\frac{1}{2}$ millions to 1. The greater part of this gearing could be dispensed with by using a ripple relay to operate the fixed-charge attachment.

I should like to ask the author whether there is any danger of a voltage being induced back into the ripple transmitter from the system, particularly under fault conditions. Would it not tend to flash-over at the primary terminals?

In the case of an undertaking having three points of e.h.t. supply, would it be necessary to have a ripple generator at each of these supply points, or could they be paralleled on one transformer and all operated from one set of transmitting gear (assuming, of course, that one ran pilots for the ripple supply between the various supply points)?

Where a d.c. network is supplied through a rotary convertor or static rectifier, will the ripple be transmitted through this to operate the ripple on the d.c. side? On page 828 the author mentions that the ripple frequencies must not coincide with the odd harmonics of 50 cycles. I should like to ask what will happen if the system frequency falls to 40 cycles per sec. as the result of fault conditions. Will there not be the possibility of the relays operating? Supposing, for instance, there is a predominant third harmonic on the distribution, will not the relays operate?

Mr. P. d'E. Stowell: One of the disadvantages of the series injection method is that in the majority of actual networks it is quite impossible to signal on only one feeder at a time, since all the feeders are interconnected on the network; neither is it easy in a number of cases to signal over them in groups.

The installation of the necessary injection transformers in the feeders can also be difficult to arrange and may be a very expensive matter, particularly when it is realized that practically the whole of the equipment has to be provided to operate even the first relay installed. As far as I can see there is no means of putting in the equipment in stages, and I think that to be able to do so would be a very considerable asset.

It seems that a good method is to install local transmitters in each substation, of the type shown in Fig. 1 or preferably Fig. 2 of the paper, operating on the low-voltage network only. They can be installed one by one as the necessity arises, and will need to have a relatively

* Paper by Mr. H. PURSLOVE BARKER (see vol. 83, p. 823).

small output since they are not operating in parallel with the load. In spite of the possible trouble due to earth faults, these methods are known to operate satisfactorily on the normal size of low-voltage network section. Initially they would be operated by time switches, until centralized control was justified, when they could themselves be controlled by an injection over the high-voltage network, using special sensitive thermionic-valve-operated relays to receive the signal in the substations and pass it on to operate the local transmitters, or by other means of control.

I have seen such a transmission injected by the parallel method shown in Fig. 6 into a 6·6-kV network carrying some 35 MW of load, by applying the signal between two phases of the low-voltage side of one of the step-down transformers, series condensers being used to cancel the transformer reactance so that the maximum ripple voltage is developed across the high-voltage side. Injection by this method obviates the undesirable feature of having to connect apparatus at a point where the available short-circuit power is high. It is not even necessary to make the injection at the power station itself.

In this particular experiment the injection was made out on the network, and about 500 watts, at a frequency of 3 000 cycles per sec., was used. Satisfactory signals were received all over the network, being taken again from the low-voltage side of the transformers across the two phases on which the transmission was being made, although half the voltage also appeared across each of the other two combinations.

In several cases the only circuit from the transmitter to the receiver was via the power-station busbars with their heavy parallel load, which did not appreciably affect the result, though it is interesting to note that a series reactor of 0·25 ohm at 50 cycles per sec. completely suppressed the signal.

The frequency of 3 000 cycles per sec. was found, after some testing, to be the optimum value in this instance, mainly owing to the presence of interference of comparable voltage at lower frequencies. The attenuation of the signal did not appear to increase very much as the frequency was raised up to this value, at which a signal-to-interference ratio considerably greater than 10 to 1 was readily obtained; and with a valve-operated relay this is the only factor of real importance.

Mr. A. P. Robertson: The wave-form distortion shown in Fig. 5 looks very big, and may not be drawn proportionately. If the distortion only lasted for a short period it might not be detrimental in the majority of instances, but I should like to know whether it has any effect on the commutation of rotary convertors. If the distortion lasted for 5 sec., for example, it might quite well be that it would cause the rotary convertors to flash-over. Some years ago I had serious trouble on rotary convertors owing to harmonics, but the harmonics were persistent and the peak voltage was raised. The distortion at that time was sufficient to cause the machines to flash-over.

Mr. H. M. Stronach: Most of the previous speakers have rather stressed ripple control of two- and three-rate meters and street lighting: in my view one should take a much broader view of this subject. The transfer of recorded units from one dial of a meter to another only alters the price at which the load may be charged, but does not get rid of the load; whereas the term "load control" denotes the definite shedding to suit station operating conditions of relatively large blocks of space- and water-heating loads.

I notice that the supply to the ripple alternator is taken from the main busbars. Is this altogether correct? Load control can be most valuable when conditions are tending to become abnormal in a generating station, with voltage and frequency falling. Should not the ripple alternator be driven by a d.c. motor supplied from an entirely separate source, e.g. a battery? In view of the author's explanation of the troubles likely to arise, should ripple control be attempted when the frequency of the main supply to the ripple alternator is abnormal?

In the event of major trouble on the system, say a total shutdown, is it possible to get rid of the controlled load before making the feeders alive, the ripple supply coming from a separate source?

Finally, has any trouble been experienced on rotary convertors when the control has been applied under abnormal frequency or voltage conditions?

Mr. W. J. Cooper: I should like to put the following questions: First, what is the capacity of a ripple alternator in relation to the size of the system on which it is to work? Second, when a supply is taken from a bulk-supply authority by several small undertakings, can the ripple be confined to a single small undertaking?

Mr. H. Purslove Barker (*in reply*): Firstly I should like to thank the speakers for the many interesting questions which they have raised on this paper. A certain number of these questions may have their origin in a lack of clarity in the paper itself, but in answering them I am in some cases referring the speakers back to the text of the paper in the hope that a re-reading may answer their questions more fully than I could do in a necessarily brief reply.

Mr. Babb correctly observes that, to be useful, any form of centralized control must be truly centralized from the central station or bulk-supply substation. Ripple control exactly complies with this requirement, and it is a fundamental feature of the system that a single transmitter can cover networks of almost unlimited geographical extent, provided such networks are fed from the generating station or substation in which the transmitter is located.

Mr. Babb asks why the growth of the system reduces the impedance, but I think he is confusing the impedance of the cables themselves (which clearly increases with length) with the phase-to-phase impedance of the whole network as measured in the central station. This latter impedance clearly gets smaller as the network gets larger. When there is no load there is infinite impedance. When there is infinite load there is no impedance between phases.

With regard to the employment of a Ferranti voltage-regulator to secure a uniform ripple, there are some practical difficulties in so doing, and the method now employed is cheaper and simpler.

Mr. Babb questions the possibility of a ripple from one network interfering with relays in another network sharing a common source of bulk supply. I think it will be found that this difficulty, which is fundamentally non-existent, is explained on page 829 (vol. 83).

He also asks whether a Petersen coil can affect a ripple system. In general it cannot, and, if at all, then only in a favourable sense.

Mr. Stevenson wonders whether ripple relays could have been operated by the harmonics which were prevalent in the early days of the C.E.B. system. Ripple relays as now designed cannot be operated by vagrant harmonics, since these are generally confined to odd multiples of 50 cycles, which frequencies are always avoided in planning ripple-control schemes.

I am in complete agreement with Mr. Stevenson in his suggestion that the advent of ripple control enables many maximum-demand problems to be solved. It is interesting to remark that several undertakings are now adopting tariffs for water-heating and space-heating by which the load is interrupted for 20 or 30 hours per annum at the supply company's discretion, thereby relieving them from the maximum-demand increment incurred by these loads. This technique is not only of negligible inconvenience to the consumer but highly profitable to the undertaking, particularly if it takes a bulk supply from the C.E.B. or another undertaking.

Ripple-control installations recently put into service have demonstrated the large economics which can be effected in water-heating and space-heating, which render the case for their adoption so demonstrably profitable that it is hardly possible to believe that the technique will not become very widespread in the near future.

Mr. Clark makes a suggestion that ripple control could be employed for the operation of fixed-charge collector attachments on meters, whereby the synchronous motor and the high-ratio gear train associated with it could be dispensed with. Such a meter is in existence in a prototype stage, and it provides the additional benefit that if ripple impulses are transmitted at intervals the rate of fixed-charge collection can be proportionately increased or decreased over a whole area by increasing or decreasing the time between the emission of the actuating ripple signals.

Regarding the possibility of voltage being induced back into the ripple transmitter under fault conditions, this problem has been entirely solved by the employment of an air-gap type transformer and by the aid of special provisions in the equipment itself. Ripple transmitters are so designed that no damage can result if a fault short-circuit occurs on a feeder at the actual moment when the ripple is being transmitted and when the transmitter is inductively coupled to the system.

Regarding the application of ripple control to an undertaking having a 3-point e.h.t. supply, it is impossible to give a general answer to the question of how many ripple transmitters would be required. It depends on the characteristics of the network.

The ripple signal cannot be transmitted through a rotary convertor or static rectifier, for identically the reason that the alternating current itself is not transmitted through such devices, and consequently the ripple injection must be separately effected on to a d.c. system at the low-tension side.

If a system frequency were to fall from 50 cycles to 40 cycles, it is certainly true that mal-operation of the relays might result, but only if a synchronous-motor drive is employed.

Mr. Stowell gives an interesting account of an experiment with parallel transmission from the high-voltage side using very low powers and presumably high frequencies. This technique is known to be highly capricious and, despite recent claims to the contrary, can be ruled out as impracticable in 99 cases out of 100. In the hundredth case there are most severe difficulties to be encountered which are not always appreciated until the equipment has been in service under all the varying conditions which are met with.

Mr. Stowell truly observes that it is very often necessary to install the complete transmitter before the first relay can be operated, but this hardly seems a serious objection.

Mr. Robertson comments that the wave-form in Fig. 5 appears to be considerably distorted. In fact, the figure, although not accurate, is not far off the truth. No trouble whatever has been experienced in the effect of such a ripple on the commutation of rotary convertors, and it is difficult to see how a ripple of this amplitude could cause any noticeable effects unless the peak voltage were very seriously raised. Mr. Robertson cites a case in which commutation troubles due to harmonics occurred, but I would hazard the guess that the peak voltage must have been raised by at least 20 %, a figure far greater than the increase due to a ripple signal.

I agree with Mr. Stronach that the application of ripple control to 2- and 3-rate meters and street lighting comprises only a very small part of the potentialities of the system. In fact, I feel that 2-rate metering is a fundamentally inappropriate method of charge, for if the ratio of price between peak units and running units is to be made anything like equal to the ratio of costs of maximum-demand units and running units on grid tariffs, such a metering system would amount to compulsion upon the consumer to interrupt supply.

The object of a peak-restriction scheme can never be to exact from the consumer the cost of the maximum demand which he occasions, but to prevent him from occasioning any demand at all, both in his own interests and in those of the undertaking. This principle can clearly only be applied to certain classes of load, of which space-heating and water-heating are the principal examples.

Regarding the supply to the ripple alternator, it is quite practicable to take it off the station battery, but hardly economical to do so, as few undertakings appear to think that the operation of the transmitter under shut-down conditions is of any moment. In practice it might be difficult to operate the transmitter at all as circuit-breakers would have tripped, thereby isolating the ripple from parts of the system where control would be required.

Mr. Cooper asks how the capacity of the ripple alternator is related to the size of the system on which it is to work. While there clearly is such a relation, its expression in simple terms is complicated by a number of factors concerned with the layout rather than the power of the network. In general the rating of the ripple machine varies from 0·1 % to 0·75 % of the system maximum demand.

Mr. Cooper's question regarding the localization of the ripple is dealt with in my reply to Mr. Babb.

INSTITUTION NOTES

DISCUSSION ON "THE PROPERTIES AND TESTING OF HEAT INSULATING MATERIALS"

The Joint Committee on Materials and their Testing is organizing a discussion on "The Properties and Testing of Heat Insulating Materials," which will be held on the 23rd November next in London, in conjunction with the Autumn Meeting of The Institution of Gas Engineers. The discussion will be divided into three sections, namely:—(1) High-temperature insulating material; (2) Low-temperature insulating material; and (3) Insulation of buildings, and other applications. It is intended that the papers shall review the present position of research and current opinion, not only in Great Britain, but also on the Continent and in the United States of America.

HIGH-TENSION CONFERENCE, PARIS

The Tenth Meeting of the above Conference will be held in Paris from Thursday, 29th June, to Saturday, 8th July, 1939. It will deal, in particular, with recent progress in the following branches of the electric supply industry:—

- The construction and maintenance of apparatus used for generation, transformation, and circuit rupture.
- The construction, insulation, and maintenance of overhead lines and underground cables.
- The operation, protection, and interconnection of networks.

Full particulars may be obtained from the Secretary, British National Committee of the Conférence Internationale des Grands Réseaux Électriques, 15, Savoy Street, London, W.C.2.

ELECTIONS AND TRANSFERS

At the Ordinary Meeting of The Institution held on the 9th March, 1939, the following elections and transfers were effected:—

Elections

Member

Rosenberg, Emanuel, Dr.Tech.Sc.

Associate Members

Affleck, Denis Boyes.
Brett, Sidney Ingarfield, B.Sc.(Eng.).
Coates, George Harold.
Cook, Leslie Bradford.
Crosse, Ernest.
Gibson, William Walter M.
Maddock, Alan Julian, M.Sc.
Pope, George Frederick.
Rigby, Fred.
Siddhu, Hazura Singh.
Stanley, Vivian Ernest.
Storey, Arthur Lambton.
Thomas, Arnold Harrison, B.Eng.

Associates

Cooke, Maurice Wyndham.
des Forges, Headley Pearson.
Dickinson, Sydney.
Didier-Garnham, Hubert Ernest.
Grover, Albert John.
Mentasti, John Felix.
Wilson, James.

Graduates

Beverley, Walter, B.Sc.
Brash, George Melrose, B.Sc.
Brecknell, William Alan.
Franklin, Ernest, M.Sc.
Highcock, John, B.Sc.
Hutton, John George, B.Sc.
Jackson, Albert Edmund, B.Eng.
Lukacs, Imre, Dipl.Ing.
Martin, Alexander James, B.Sc.(Eng.).
Painting, William Arthur E.
Patchett, George Henry.
Pearce, Frank Owen, B.Sc. (Eng.).
Price, Edward Michael, B.A.

Students

Ahmad, Zaheer.
Baird, Alexander Innes K.
Batt, Sidney John.
Baulk, Roland Hugh.
Bhadha, Peshotan Maneckji.
Borup, Arno.
Brewster, Sydney.
Bourne, Walter John.
Brinkworth, Roland Riddle.
Brown, Harry.
Burgess, Lewis Brian.
Buzza, Herbert.
Cameron, James Dalziel.
Canfor, Ronald James.
Carsbury, William Henry.
Cartwright, Albert Edward.
Chalmers, Gilbert Ramsay.
Chaplin, Jack James.
Chatterton, Hugh.
Clark, James Randall E., B.A.Sc.
Coles, Douglas Harry, B.Sc,
Cowell, Albert Charles.
Cowley, Frederick Thomas.
Dandekar, Ramchandra Kashinath.
Davie, Owen Hosmer.
Dixon, George Wilfred.
Dransfield, Desmond.
Drury, James.
Dunsford, Kenneth Martin.
Durston, David Stanley.
Edwards, Eric Thomas A.
Entwistle, Arnold Geoffrey.
Evans, John Vincent.
Farrow, Dudley Selwyn.
Ferguson, Stuart Grantleigh.
Ferrer, Geoffrey William.
Fitzpatrick, Henry Walter P.
Gilbert, Thomas.
Gilmour, Ronald Robert.
Goodship, Geoffrey.
Gregory, William Langdale.
Halfter, Harold Paul.
Hamer, Arthur.
Hankins, Frank Albert L.
Harper, Bernarr Charles.
Harper, Harry.
Hart, Francis.
Hartley, Frank.
Hedges, Peter Octavius.
Hicks, Archibald Jack.
Hoare, Gordon Francis.
Holcroft, Norman.
Howe, Frank.
Howshall, Eric.
Hurst, William.
Irani, Salamut Noshirwan.
Jackson, Barry Cornish H.
Jeans, Gerald Leslie G.
Jeffereis, Charles Deryck.
Jerome, Kenneth Wilfrid.
Jhala, Kumarshree Jayvirsinhji B.
Jones, Ernest Anthony Sadler.

Students—continued.

Jones, Leslie Llewellyn.
Jones, Maurice Clement.
Judge, Walter Hagland.
Kearn, Geoffrey.
Keins, Herbert Adolf.
Kempner, Peter Gerhardt.
Khan, Abdul Hameed.
Khurana, Bhagwan Das.
Kime, Donald.
Leaning, Anthony Tennant.
Lobo, George.
Looser, Robert Conrad.
Low, Robert Hutcheon.
Mahapatro, Tarini Charan.
Mallikarjunarao, Ketavarapu.
Markham, Douglas Hale.
Marshall, Andrew Gardner.
Marshall, Ronald Sidney.
Martin, John Keith.
Martin, Joseph Edmund.
Mason, Sidney James.
Mellows, Ernest Allenby.
Miller, Archibald Cochrane.
Mirchandani, Arjan Jethanand.
Moden, John Carol.
Moore, Stephen.
Mullett, David Henry.
Murray, James Hugh.
Newsome, William Antony.
Nicholas, Tom William C.
Outhwaite, Robert.
Page, Philip Theodore V.
Pai, Mangalore Srinivas.
Panicer, Kehar Singh.
Partington, Edward.
Patel, Indulal Prabhudas.
Phethean, John Forshaw.
Postlethwaite, Peter Colin.
Powell, Edward Bertram.
Probert, Howard John.
Quarmby, Raymond Barber.
Qureshi, Mumtaz Husain.
Rajagopal, Ponnusamy.
RamachandraRao, Rentala.
Ramasubbu, Subrahmanya.
Ranade, Manohar Narayan.
Rashid, Mohd Abdul.
Russell, Victor.
Savage, James Robert.
Schwarz, Konrad.
Schwitzer, Kurt R.
Shaw, Maurice.
Smith, John.
Spencer, Herbert Cyril.
Stamps, James Herbert.
Stead, Harold Peel.
Stockwell, Aston Joffre.
Sulter, Frederick James.
Symons, Ronald.
Taylor, Robert.
Thompson, William Alexander.
Thornley, Josiah.
Turtill, Robert Henry.
Tyack, David Reginald.
Upton, Peter Richard.
Vadgaokar, Manohar Ganpatrao.
Vaidya, Balkrishna Jagannath.
Walker, Victor Kenneth.
Wash, Geoffrey Henry.
Watson, Archibald Dick C.
Way, Brian Oscar.
Weaver, Geoffrey Edgar.
Webster, William Abram.
White, Lewis John H.
Wilkinson, Norman Edmund.
Willis, Alfred Reginald.
Wilshaw, Arthur Anthony.
Wiser, Allan.
Yates, John Thomas.
Zaidi, S. Fayyaz H.

Transfers

Associate Member to Member

Bird, William.
Brough, George Hall, B.Sc.
Hogg, Duncan Bardsley.
Hope, Vernon.
Kirke, Harold Lister.

Associate to Associate Member

Lythall, Reginald Tarlton.
Shaw, Horace.
Short, Arthur Henry.
Whyte, Robert Paisley.

Graduate to Associate Member

Abrahamson, Arnold Mogens, B.Sc.
Baird, Robert Alexander, B.Sc.
Bearcroft, Hubert Percival.
Brown, Cyril Dykes.
Burnet, James.
Coutts, James Arthur.
Dawson, Stanley.
Faux, Frank.
Finch, Kenneth William, B.Sc.
Fisher, Francis Llewellyn M.
Gemmell, William John A., B.Sc.
Griffiths, Eric William S., M.A.
Haseler, Alfred Ernest.
Hounsfield, Robert Brailsford, B.A.
Johnson, Winton Thomas, B.Sc.
Lackey, Clemett Harrison W., B.Sc.(Eng.).
Laycock, William Edward.
Lewis, Harold.
London, Peter.
McIlhagger, David Sherwood, M.Sc.
Maloney, Stanley John.
Myers, David Milton, B.Sc., D.Sc.(Eng.).
Palethorpe, Wilfred, B.Sc.
Rees, John Glyn, B.Sc.
Richardson, Robert Burgon.
Ricks, Bertie William.
Soper, Percival Frederick, B.Sc.(Eng.).
Teal, Arthur Edward, Lieut. R.E. (A.I.R.O.).
Tudor, William Cowing, M.Sc.
Twiss, Victor Benedict, M.A.
Walker, Frederick Alfred S.
Williams, Francis Edward, M.Sc.(Eng.).
Wilson, William Eric, B.Sc. Tech.

The following transfers were also effected by the Council at their meeting held on the 30th March, 1939:—

Student to Graduate

Ainslie, Kenneth Oliver, B.Sc.(Eng.).
Arup, John Dahl.
Austin, George Nicol, B.Eng.
Balean, Richard Masters.
Barlow, Kenneth Frederick, B.Eng.
Beresford, Donald Alfred.
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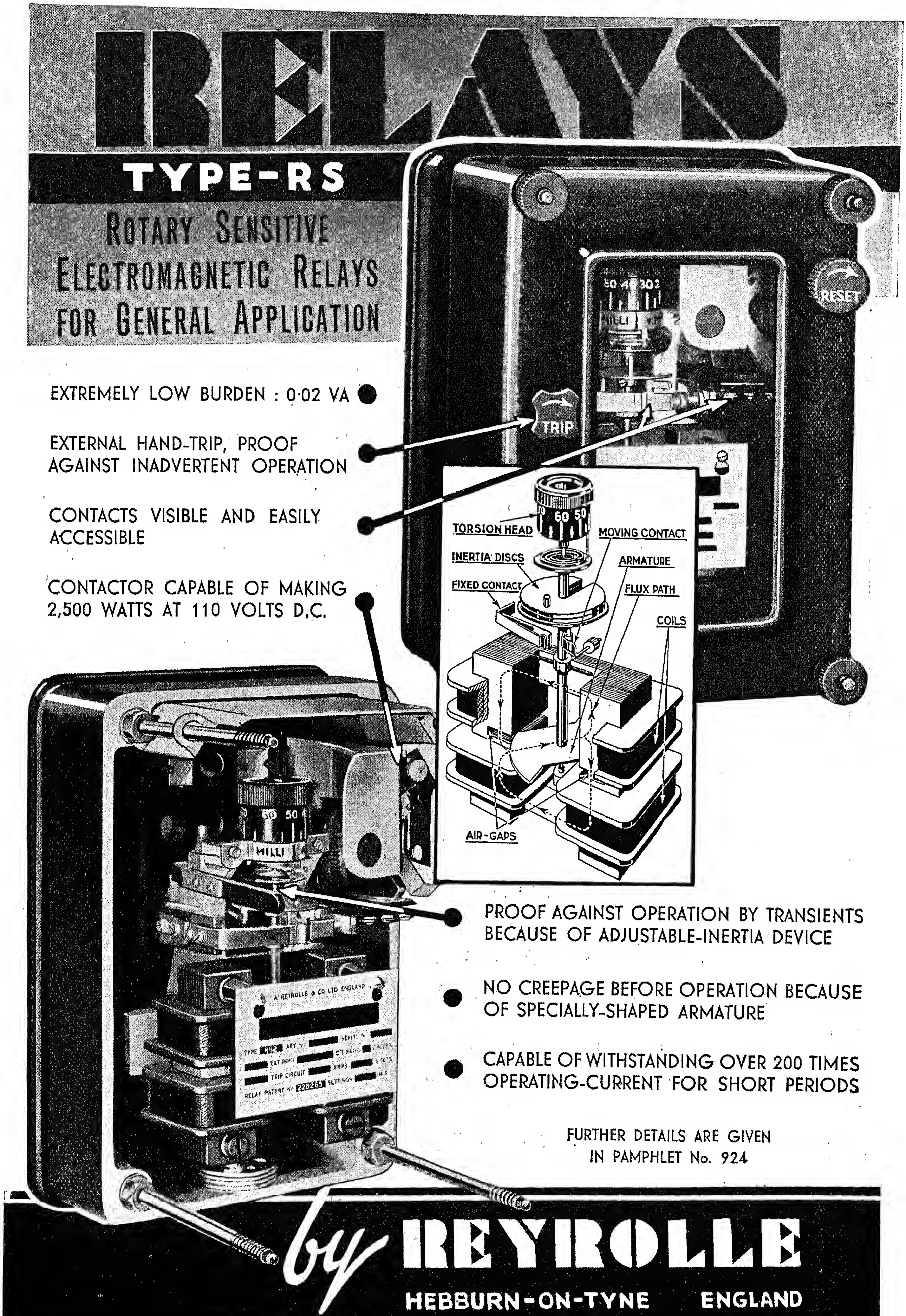
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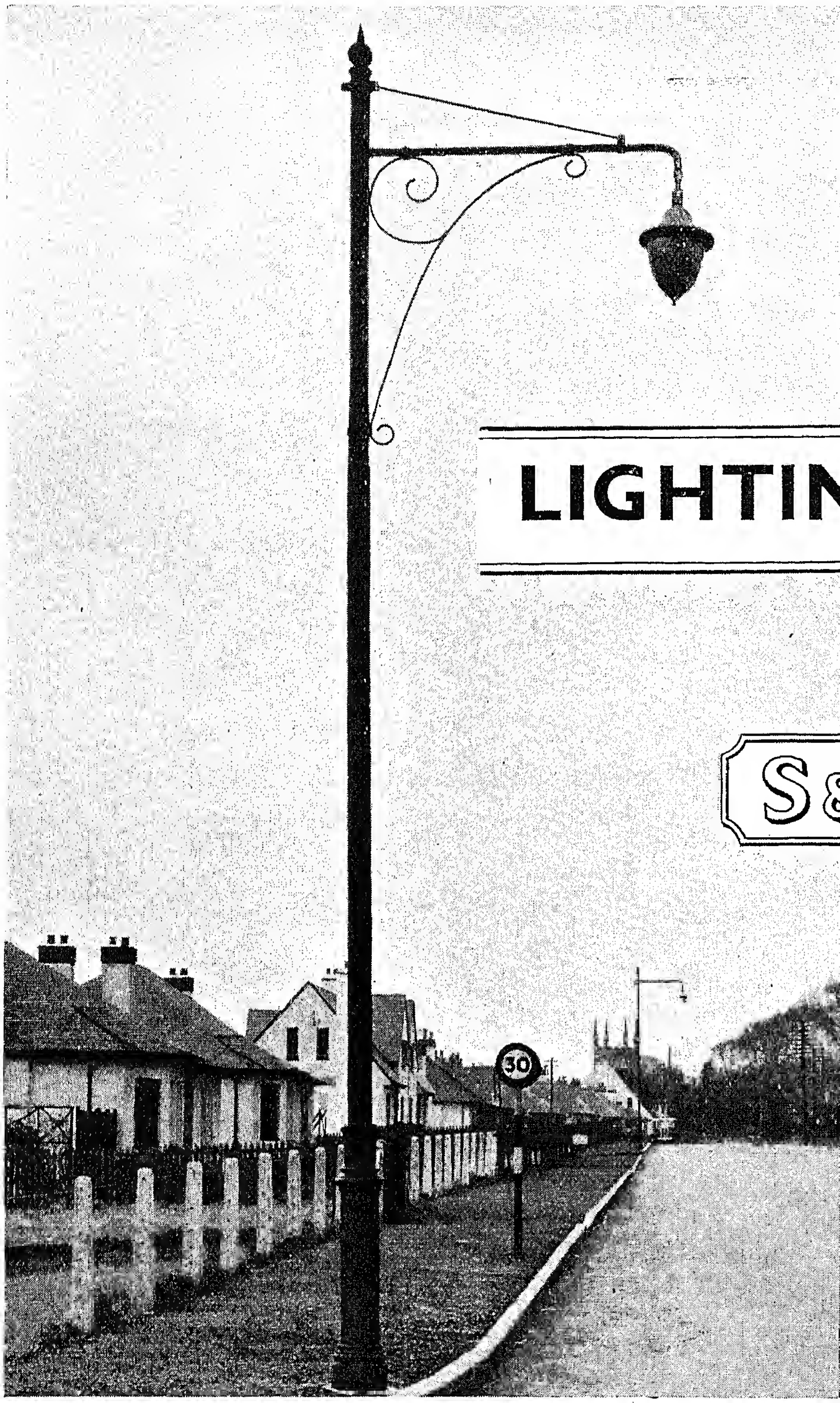

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